FALL OF THE TAY RAILWAY BRIDGE.

A TREMENDOUS disaster, the destruction of a grand new work of railway engineering and the sudden death of nearly a hundred persons, took place on Sunday evening, Dec. 28, 1879, at the estuary of the Tay, just below the town of Dundee. The central part of that imposing structure was literally blown away by a gale of wind last Sunday evening; thirteen of the lattice girders crossing the middle spans of the bridge were all at once torn up from the summits of the iron and brick piers that had supported them, as the train, passing over the bridge from the southern to the northern shore, presented its broadside surface to the violent westerly gale. Every carriage, as well as the whole of the upper ironwork in that part, a length of three thousand feet above the deep navigable channel, was hurled into the river, and every person in the train was either drowned or otherwise killed.

The project of crossing the estuary of the Tay by bridge had for many years been discussed, but it was not until 1870 that the North British Railway obtained powers under an Act of Parliament to carry the idea into effect. In the keen competition of rival railways, the saving of twelve miles in the length of route between London and Edinburgh in favor of the Great Northern and North-eastern system over the London and North-western and Caledonian system, had been the first object. The shortening of the distance by twenty-six miles between Edinburgh and Aberdeen was another; and, finally, the advantage to Dundee of bringing the coal fields of Fife into communication with the ships bringing jute into its port was not the least of the considerations which led to this undertaking. The Tay is at the site selected, about two miles above Newport, nearly two miles in breadth, with a maximum depth at high water spring tides of some 45 feet, and a velocity of current at times of as much as five knots an hour; and in order to clear the masts of the shipping, so numerous at this busy commercial harbor, an elevation of 88 ft. above the river was needful as the primary condition of the level at which the structure had to pass over the central portion or waterway of the river.

Such were the conditions which Sir Thomas Bouch, a Scottish engineer, was engaged to surmount, and, in view of the necessarily great cost of so extensive an undertaking, lattice girder construction and a single line of rails were determined upon. The general aspect of the work may be described as consisting of two long lengths of lower level lattice girder bridge works on each side, upon the top of which the rails are laid; only the high level, or central portion, over the shipway, rested at either end respectively, on the low level portions, and the trains ran through it on rails laid on the interior of its base, corresponding in level with the rest of the roadway, the total length of bridge being 10,320 ft., or 3,459 yards. The lower level portions are supported on piers of various constructions; the typical form in the original design was that of foundations of iron cylinders, filled with brickwork and concrete, finished with a stone coping, and from this base arose two circular hollow piers, of brickwork in iron casings, from 9 to 15 ft. diameter, braced together by an intermediate brick wall of three ft. in thickness. In consequence of variations in the river bed, and the various exigencies of foundation, various corresponding divergencies in construction were made, and in the central or high-level part of the bridge, the supports consisted of cylindrical brickwork bases, in iron casings, extending 40 to 45 feet in depth, with groups of cast iron columns rising therefrom, and held together by horizontal and diagonal bracings. The first contract was taken by Messrs. De Bergue & Co., of Manchester, in 1871, but on the death of the principal of that firm the execution of the work was taken in hand the following year by Messrs. Hopkins, Gilkes & Co. of Middlesborough, who brought it to completion in September, 1877 after an expenditure of £350,000, and in February of the following year the road was passed for traffic by the Inspector of the Board of Trade, who was Major-General Hutchinson, R.E. The bridge, as finished, consisted of a total of eighty-five spans, all of which were of lattice girder construction, except one, which we shall specially notice. Commencing from the south end, the first eleven are of 245 ft. each, the next two 227 ft. each. Then comes one span of 166 ft., in which the bowstring principle of girder is adopted. Then followed lattice-girder work again, the next span being 162 ft ; then thirteen spans of 145 ft. each, ten of 129 ft., eleven of 129 ft., two of 87 ft., twenty-four of 67 ft., three of 67 ft., one of 66 ft., and six of 29 ft. each respectively. In addition to the above there are adjoining the north end one span of 100 ft. bowstring girder, and three spans of 29 ft. of plate girders. In the fifteen spans exceeding 145 ft. and in the 100 ft. bowstring girder wrought-iron cross girders were employed; but in the other spans cross girders of timber. The lattice girders were arranged in continuous groups of from four to six in a group, and provision was made for their expansion in hot weather. The piers from one to fourteen spans are entirely of brick in cement, but from fifteen to forty-eight spans they are brick to five feet above high-water mark, finished with stone belting, upon which are carried groups of cast iron columns braced together. The spans seventy-eight and seventy-nine have cast iron cylinders filled throughout with concrete. Thence to the eighty-fourth span are cast-iron columns, and from the eighty-fifth to the eighty-ninth they are brick in cement. The materials used in the entire work were 7,200 tons of iron, of which nearly half, or 3,200 tons, were castings, 87,000 cubic feet of timber, 15,000 casks of cement, and some ten millions of bricks. What strikes one forcibly in these details is the variety both in the materials and in the structure, and what also is remarkable, in appearance of the rectangular form of the girder bridge, is its height compared with its breadth. The inclines of the bridge railway are not excessive, being 1 in 74 on the north side and but 1 in 356 on the south side. The highest altitude of the bridge occurred at the center of the large spans, where the height from river to line of rails was 130 feet. The girders of the thirteen great spans are said to have weighed as much as 200 tons apiece. The permanent way was laid with double-headed steel rails of 75 lb. to the yard run, and these were secured by fish-joints and cast-iron chairs at three feet average intermediate distances. The chairs were fastened to longitudinal timbers; the lattice girders rested on the centers of the double piers, and there was a handrail of wood along each side of the two low-level portions.

The bridge was tested by the government inspector, General Hutchinson, to a severe degree; six of the fine goods locomotives of the North British Company, each weighing 73 tons, and each measuring 48 feet in length—a total of 438 tons weight and 281 feet in length—were run over it at speeds ranging up to forty miles an hour, the deflections of the long 227 and 245 foot girders being only 1.8 to 1.2 inch, and the shorter girders only deflecting 0.9 to 0.6 inch. These results were pronounced by General Hutchinson at the time as "very satisfactory." The lateral oscillations as observed by the theodolite were also very slight, and the structure altogether showed great stiffness. By the direction of the inspector, the working speed over the bridge was limited to only twenty-five miles per hour; and the single line was to be worked by train-staff and block system; and he also advised very careful attention to be paid from time to time that no scouring action should be allowed to go on at the foundations of the piers. Everything, therefore, at the open-

### THE TAY BRIDGE ON THE MORNING AFTER THE DISASTER
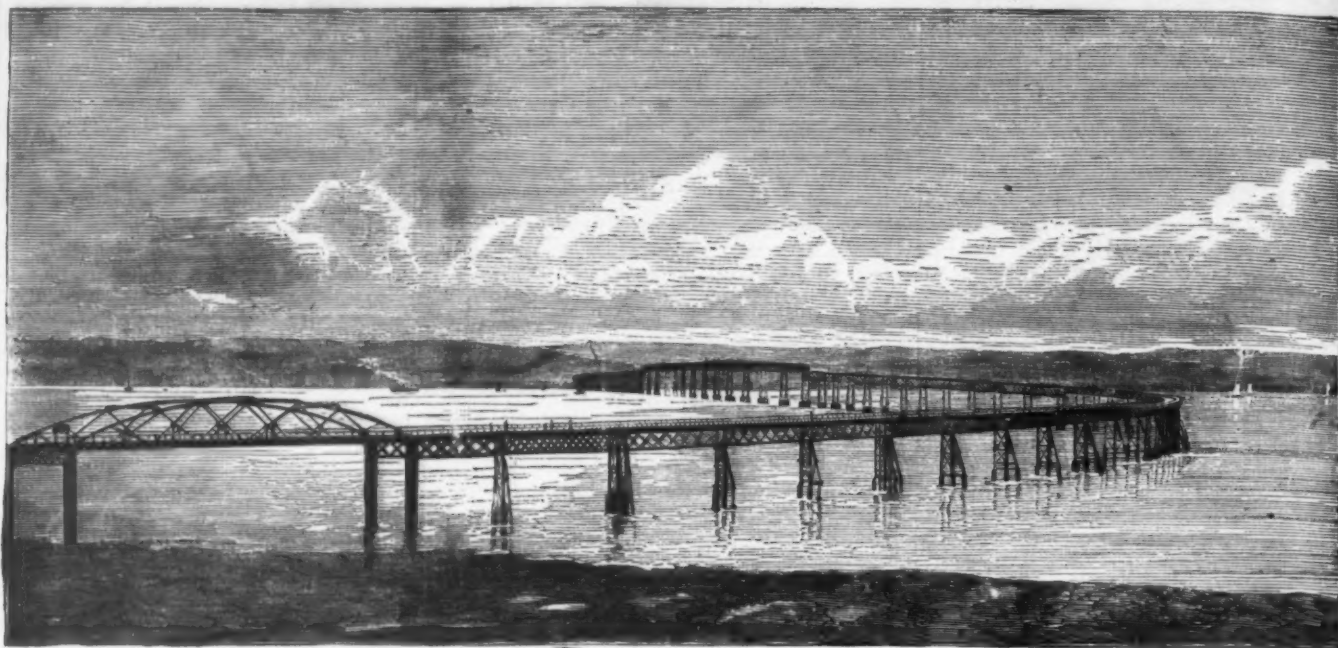
VIEW OF THE TAY BRIDGE FROM THE NORTH.

ing of the bridge in May last year (1878) seemed to be satisfactory, and we must look to the evidence hereafter to be brought forth for an explanation of its failure in stability. Lord Sandon has sent two of the railway inspectors, Major-General Hutchinson and Major Marindin, to ascertain all particulars that can be obtained with regard to the accident; and he has also directed that a formal inquiry, under the provisions of the Regulation of Railways Act, 1871 (34 and

nearly all the wayside stations. On arrival at St. Fort, the last station before reaching the Tay Bridge, the train was found to be five minutes late. Here the tickets were collected, and, at thirteen minutes past seven o'clock, according to the usual custom, the signalman, Thomas Barclay, stationed at the south end of the bridge, handed to the stoker the baton, without which no train was allowed to cross. At this time the gale was blowing with such violence that it was

the lights being distinctly visible until the locomotive entered between the high girders in the center of the bridge. This, as we saw, was so constructed that trains ran on a level with the top of the girders until the central spans were reached, where, in order that the navigation of the river might be facilitated, the rails were placed on a level with the bottom of the girders, thus giving a number of feet additional height to allow of vessels passing under on the way
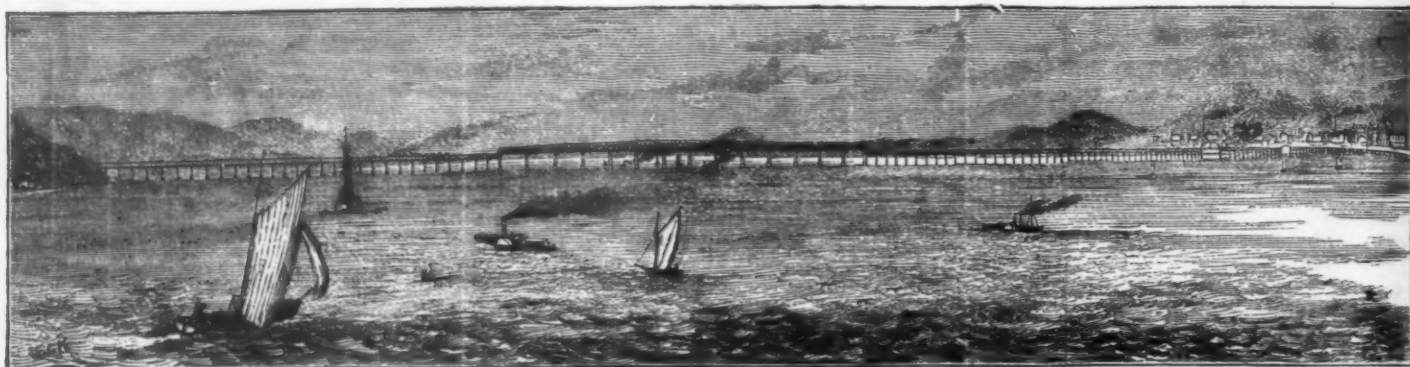

TAY BRIDGE FROM THE FIRTH, LOOKING UP STREAM.

35 Vict., cap. 78) shall be held with the least possible delay. The inquiry will be conducted by Mr. H. C. Rothery, Wreck Commissioner, Colonel Yolland, Inspector-General of Railways, and a civil engineer.

The train from Edinburgh to Dundee, which encountered this terrible disaster, consisted of four third-class carriages, one first-class carriage, one second-class carriage, guard's brake and engine. It left Edinburgh on Sunday afternoon at 4:15, stopping, as is usual with many Sunday trains, at

with great difficulty that Barclay regained his cabin. Along with him in the cabin was a surface-man named Watt, who expressed great doubt as to the security of the bridge. Together, accordingly, the men watched the train as it passed along at the usual rate of three miles an hour. The moon was shining brightly, although the wind was blowing a fearful hurricane, the white-crested waves in the Forth and the damage caused on shore testifying to its violence. The lookers-on continued to watch the progress of the train, all

to Perth. It was just after the train had passed from the upper to the lower line of metals, between the high girders in the center of the river, that a fearful blast, with a roar resembling a continuous roll of thunder, swept down the river. Some of the spectators state that at that moment an intensely brilliant sheet of flame and a shower of sparks were seen at the high girders, caused by the fractured iron as the massive structure broke and fell into the seething waters of the Tay. The signalman and his companion at the Fife end
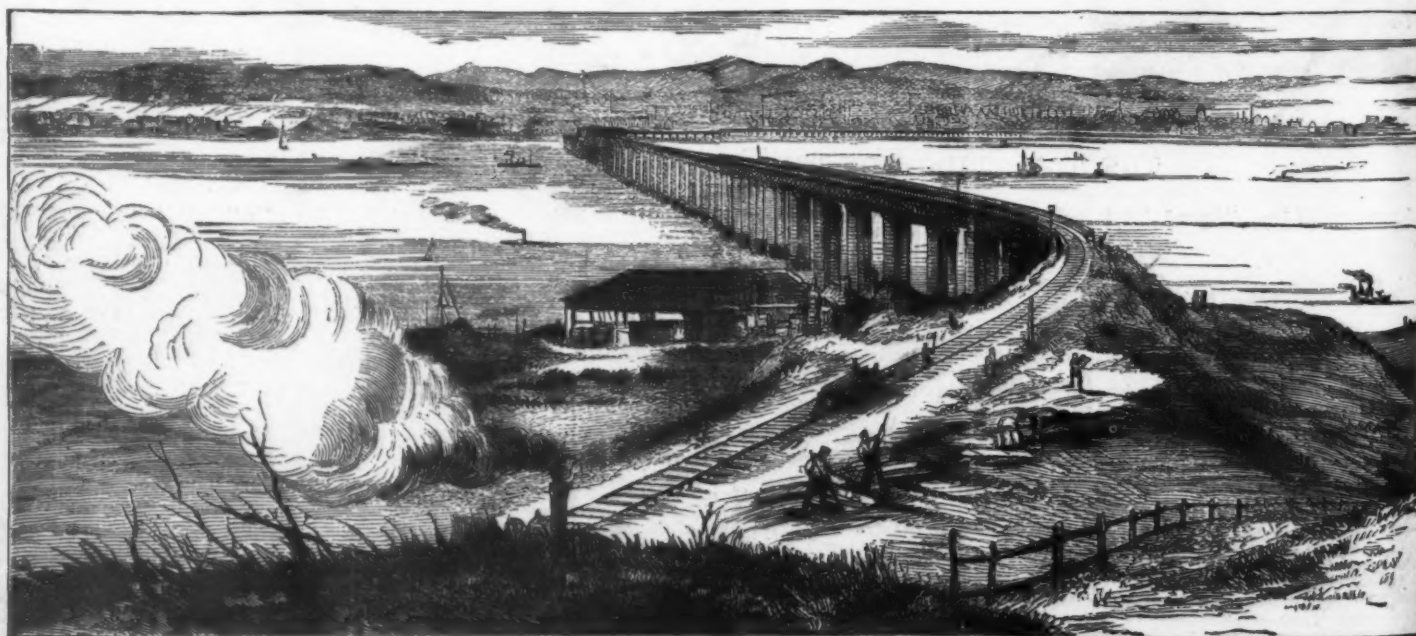
VIEW OF THE TAY BRIDGE FROM TAYSIDE

of the bridge did not see so much. From their position, on the level of the rails and looking along the line, the red lights at the rear of the train were clearly seen for a considerable time, until the men calculated the engine must have cleared the high girders forming the central part of the bridge. Then the lights suddenly disappeared. Barclay thought the train had rounded the curve, but Watt was apprehensive that the bridge had given way, and on Barclay proceeding to the telegraph instrument the dread suspicion was too fully confirmed. By neither of the eight wires which led into his cabin could a signal be obtained. The first impulse of the men was to follow the train across the bridge, but they were unable to face the furious gale; they walked along the shore accordingly for some distance, and soon were able to discern by a sudden gleam of moonlight the great gap in the bridge. They made their way back as quickly as possible to Tay Bridge station, and endeavored by repeated signaling to ascertain whether the driver of the train from Edinburgh had noticed that the bridge had given way, as they had seen a red light across the broken portion of the structure, and so they hoped that he had noticed the fearful chasm and had been able to check his train and put back to the Fife shore. All their efforts to call the attention of the signalman across the Forth were, however, unavailing, their telegraphic signals meeting with no response whatever, and the railway officials were forced to the conclusion that the train, with all its living freight, had fallen or was blown into the river. The people awaiting their friends at the railway station became alarmed, and when an intimation of the accident was given the greatest excitement prevailed. The evil news passed with proverbial rapidity throughout the district, and in a short time crowds of the inhabitants had assembled at the Harbor. The Provost and other municipal officials joined with the railway authorities in devising means for ascertaining the full extent of the disaster. A powerful steamer was got under way shortly before eleven o'clock, and proceeded out into the Forth.

The current, however, was so strong and the force of the southwesterly wind so great that it was not until they were close to the wrecked bridge that those on board the steamer were able to see what really had occurred. Then it became painfully evident that the whole center of the bridge had disappeared, the line of foam between the piers marking where the wreckage lay, and where, probably, many of the unfortunate passengers were lying in death. The steamer returned to shore, and soon, the melancholy news being made fully known, the wildest excitement prevailed. Those who had relatives in the train gave way to outbursts of grief, while many could not realize the extent of the calamity. Throughout the night the utmost consternation prevailed. At seven o'clock next morning the Tay Ferries steamer, with several gentlemen on board, visited the scene of the disaster. The harbor diver was also on board, and his apparatus was fixed to a barge, which was towed behind the steamer. When the vessel reached the bridge a boat which had been put off by Captain Scott from the Mars came alongside with sounding leads and long poles, and the depth of the water around the scene and the gaps between was sounded, but no bodies were found, and no traces of the sunken train were visible. The diver went down at two places, one near the south end of the large foundations, and he found the girders lying on the bed of the river, though there was no appearance of the train. At the second spot there was about six and a half fathoms of water, but nothing could be done owing to the water being muddy. The steamer returned to the Craig Harbor, and arrangements were made for a second visit later in the afternoon. Several boats from the Mars remained about the spot dragging the river, but nothing was recovered, although some rags and parcels were seen floating near. The body of an elderly woman drifted ashore that day at Newport.

The number of lives lost was reported at first to be from two to three hundred, but it has been ascertained that all those in the train did not exceed ninety, of whom seventy-five are positively reckoned, while there may have been others, including children, not represented by tickets issued to them.

The gap left in the bridge is above half a mile wide, and twelve of the iron columns are snapped off short, falling with the superstructure. It is the belief of Mr. Carswell, engineer to the North British Railway Company, and of other competent persons, that the origin of the disaster was that one of the carriages, probably one of the last, was blown off the single line of rails with great force against the central girders, causing the elevated portion of the bridge to give way.—*Illustrated London News.*


THE TUNNEL UNDER THE MERSEY, AT LIVERPOOL.

THE works in connection with the long-promised railway tunnel under the Mersey were commenced Dec. 13, 1879, by sinking a shaft on the Birkenhead side of the river. Two shafts are to be sunk, and a heading will be driven from each at the level of the lowest part of the tunnel under the river. From these points, if nothing occurs to change the opinion of the engineers, the tunnel will be cut through solid rock to the streets of Liverpool and Birkenhead, a length of some 1,500 feet. The first contract, amounting to half a million of money, relates to the distance from Grange lane, Birkenhead, connecting the London and Northwestern and Great Western systems and the dock railways on the Cheshire side, and carrying a double line of railway under the river Mersey, terminating in Liverpool.

HUNDRED TON GUNS.

Two 100-ton breech loading guns which Sir William Armstrong is making for the Italian Government at his factory, near Newcastle, will be the largest breechloaders ever turned out. The system of the breech will be a modification of that known as the French screw relieve system—a screw through which smooth ways are cut, enabling it to be readily drawn out at a certain position, and to be tightly fastened in its place by a mere turn of a few inches. When extricated, the breech screw will be run out of the way on a small carriage, and the loading apparatus, which will be worked by hydraulic power, will have uninterrupted play. Arrangements will be made for loading at the muzzle as well as the breech by the same apparatus, all the addition necessary being a sort of trough of communication, to make up for the difference in length between the breech end and the chase or barrel. The English War Department has considered the plans of these and other breech loading guns, and a great change in the whole system of artillery is impending

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A NEW 13-POUNDER GUN.

ON the Queen's birthday the A Battery of the Royal Artillery, "G" of the First Brigade, was armed with a field gun of novel construction and extraordinary power. This new 13-pounder gun is about twelve inches longer than the ordinary 9-pounder, and yet weighs no more, being an 8-cwt. gun. It is polygrooved, having ten shallow grooves and a studless projectile, the soft-metal gas check in rear squeezing into the grooves upon the shock of discharge, and thus effecting rotation of the shot or shell. The breech end of the bore is chambered to the extent of half an inch in diameter beyond that of the bore, allowing no less than 3 lb. of powder to be employed for a full charge. Hence it is a very much more powerful weapon than the 9-pounder in every way, although requiring only four horses instead of six to draw it. The new gun is fitted with an entirely novel arrangement for elevating and depressing, as may be seen by the drawing, the means employed being a toothed "arc" working in the slot of the trail in connection with a small cog-wheel and handle. It was first tried in July last on the Black Mountains at Hay, in Breconshire, when the powers of range it exhibited were most remarkable. One degree of elevation gave 1,290 yards, but with five degrees no less than 3,000 yards' distance was covered, or a mile and three-quarters, the mean errors of range in each case being only 8 yards and 17 yards respectively. The initial velocity, too, was tremendous—1,680 feet per second, with a pressure of only twenty tons to the inch within the powder chamber. While the 16-pounder only hit the target eighteen times out
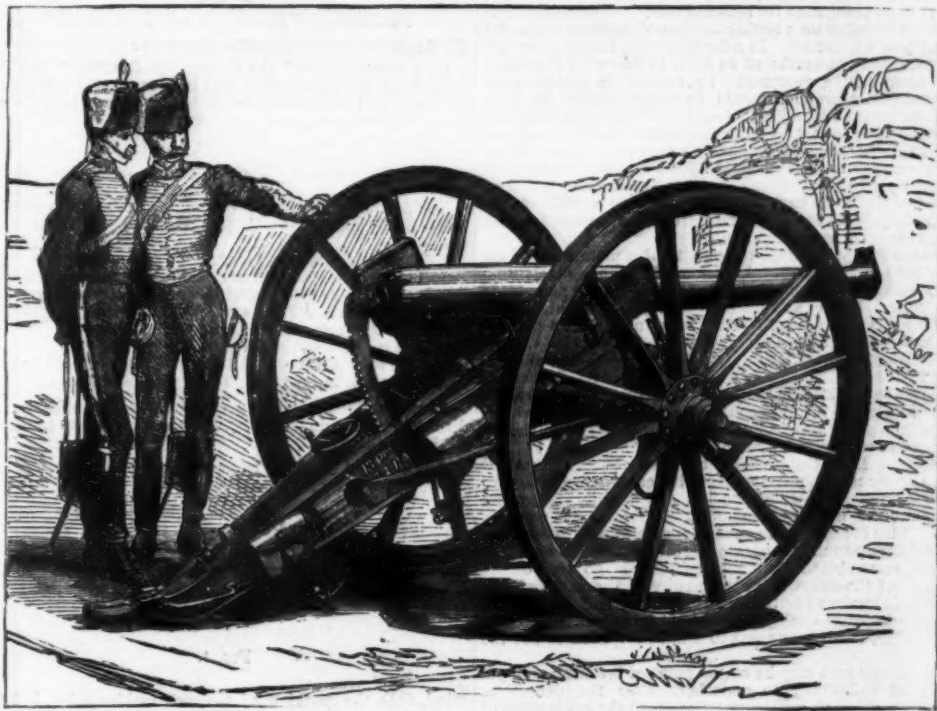
A NEW 13-POUNDER GUN.

of forty, the new field piece hit thirty-five times. Subsequently a dummy gun carriage and horses some eighteen or twenty feet long was set up at a range of 5,000 yards (nearly three miles), and was struck almost every time, the shell bursting within a distance of twenty feet either in front or rear of the object aimed at. The introduction of this novel piece of artillery has placed us distinctly in advance of the continental powers, none of whom have a weapon which can compete with it either for extent of range, accuracy of shooting, destructive power, or handiness of manipulation. Our engraving is from a photograph kindly sent us by Captain Frank Armstrong.—*London Graphic.*

NOTES ON IRONWORK.*
By GRAHAM SMITH, Assoc. M.I.C.E.

THE branch of science which enables the engineer to determine the theoretical amount of strain in the members of any proposed structure may be said to appeal directly to ordinary intelligence, and to be on the whole simple. The science, however, depends upon data and conditions the exact influence of which can never be determined in actual practice. It is proposed, therefore, in this paper to consider briefly some of the practical questions which affect theoretical deductions, and the design, efficiency, and economy of ironwork structures generally. The precise conditions under which ironwork will be constructed and worked being indeterminate, it becomes necessary, among other matters, to have some knowledge or workshop practice and routine in order to determine the proper limits and importance to assign to theoretical results. In taking out strains it is usually assumed that each member has a normal length, whatever the amount of strain to which it is subjected, and that its conditions are the same as they would be were it free to turn in a plane about its extremities. Both of these assumptions are to a certain extent erroneous. So far from any bar having a normal length, that is, being perfectly rigid, it may be taken for granted that directly any piece of iron is subjected to a tensile or compressive strain its length is varied accordingly. Likewise no member of any structure is perfectly free to turn in a plane about its extremities; were it so, each junction would have to be made with an absolutely frictionless pin. In English practice junctions are frequently made with innumerable small rivets, which render them to all intents and purposes rigid. In America, however, pin connections are employed to a very large extent, and undoubtedly with pins and eyes properly proportioned efficient joints may be made, and with simple arrangement of parts theory be more closely approached than with our complicated systems with riveted joints.

* Lately read at the meeting of the Association of Municipal and Sanitary Engineers.

Variations in the temperature of the atmosphere likewise materially affect the strains in iron structures. When constructing an iron bridge a camber is given to it, so that when loaded it may assume a straight line instead of exhibiting signs of apparent weakness by sagging. While testing the bridge it is usual to measure the camber as the load is put on, and it is not uncommon to find that on a warm day the camber is greater than it was the evening before, notwithstanding that a larger amount of load has been put upon it. This anomaly is due simply to the sun warming up the top flanges, and causing them to extend, whilst the bottom flanges have not extended to a similar extent, owing to being protected from the sun by a platform or the load upon the bridge. It has been ascertained that a variation of temperature in iron of 15° Fahr. will produce the same effect as one ton actual load per square inch; therefore a change of 82.5° Fahr. will produce the same effect as 5.5 tons per square inch actual load, which is greater than the amount of strain supposed to be put upon any bar when under its full working load. Now, although the difference between the extremes of temperature in this country may be estimated at 82.5° Fahr., the extreme temperatures only act during a short portion of each twenty-four hours, and so, owing to the mass of iron and other circumstances, the temperature of the structure is seldom the same as that of the atmosphere, consequently the iron is not affected to the full extent just mentioned. There are, of course, many positions which will at once suggest themselves where the temperature is tolerably uniform throughout the year, and where accordingly no provision need be made for expansion and contraction due to changes of temperature. In exposed positions in this country an allowance of $\frac{1}{8}$ of an inch in each 100 feet should be made if it is wished to eliminate strains which, it has been shown, may be of considerable amount. Edwin Clark has placed it on record that half an hour's sunshine has more effect on the tubes of the Britannia Bridge than the heaviest rolling loads or the most violent storms. Questions of the foregoing nature having been considered, and the strains upon the various members of the proposed structure having been determined within reasonable limits, it becomes necessary to arrange the material to meet them. It is in doing this properly and economically that the art of designing ironwork consists. In all designs every endeavor should be made to employ iron of such dimensions and weights that it may be easily procured in the open market, and require only such workmanship as may be cheaply and readily performed. By attention to these points economy will be more surely attained than by the saving in the weight of iron which may be effected by adhering more closely to theoretical refinements. As an instance of this, it may be stated that the actual weight of a plate girder is always very much in excess of its theoretical weight, and it is rarely the lightest form of girder which it is possible to design to carry a load; it is yet generally the most economical type to adopt for small spans, owing to the uniformity of its parts and the simplicity of its manufacture. While mentioning plate girders it may be well to state that, although the theoretical economical depth of all girders depends upon their description, the loads to be carried, and a variety of other circumstances, the depth of a plate girder is often fixed by one consideration alone, and that of a practical nature quite beyond the control of the designer. It is simply the fact that plates cannot be rolled at ordinary rates over 4 feet 6 inches in width, so that the maximum depth of ordinary plate girders is fixed at 4 feet 6 inches. If this depth is exceeded it becomes necessary to plate the web vertically, which will enhance the cost of the work to an extent exceeding the saving likely to result from conforming more nearly to any greater depth which theory might dictate. In arranging the flanges, although, theoretically, the section of metal should be reduced at certain points, it is generally desirable when a limited number of girders are to be made from one design to keep the plates as nearly uniform in thickness as possible, rather than to vary their thickness so as to approach more closely to the amount of metal required to meet the strain. However, where a large number of girders are to be constructed from the same design, the plates may be varied in thickness without increasing in any way the cost of the work, as the plates can be ordered in batches from the rolling mills and regulated to their respective girders in the manufacturers' yards.

At one time much of the iron employed for girder and bridge building came from Staffordshire, consequently

specifications were prepared in such a manner that iron from this district might comply with their stipulations. These specifications have been copied and recopied even up to the present time, notwithstanding that Staffordshire iron is now rarely put into ordinary ironwork, for the reason that the sizes of the iron supplied from this district are small when compared with those from the north country. This is owing to the Staffordshire mills working with plant which was put down many years since, while the ironworks in the Cleveland district are provided with more modern machinery and improved appliances. A South Staffordshire plate to cost the ordinary market rate must not be over 4 cwt. in weight, 15 feet in length, and 4 feet in breadth, and about 80 superficial feet in area; whereas Cleveland plates can be procured without additional cost up to 21 feet in length, 4 feet 6 inches in width, and 12 cwt. in weight, providing the area does not exceed 56 superficial feet. Although plates from the latter district may be obtained possessing as great a tensile strength, both with and across the fiber, as those from Staffordshire, they are not, as a rule, equal to the latter in toughness. Extra care should therefore be taken to test and thoroughly ascertain the quality of the iron, as it is sometimes very brittle; no attention whatever should be paid to "brand," as it is no criterion by which to judge of the qualities of iron usually employed for the construction of ordinary ironwork.

A very fair specification for girder iron is 20 tons per square inch and 6 per cent. elongation with the fiber; 18 tons per square inch and 9 per cent elongation across the fiber for plates; 22 tons per square inch and 9 per cent. elongation for L and T's; and 24 tons per square inch and 15 per cent. elongation for rods and bars. These elongations ought to be taken on a testing section of uniform width for a length of 6¼ inches. In a length of 6¼ inches there are one hundred sixteenths of an inch, so that each $\frac{1}{16}$ elongation after testing represents 1 per cent. In preparing all samples for testing they should be drilled out of the plate angle or bar, and be either chipped or slotted to the required dimensions, and all tool marks carefully filed out, and the parallel portions should run in with curves of large radii to the portions through which the pin holes are drilled. In the event of there being the slightest shoulder at either of these points, it will have the same effect as a nick in the iron, which will generally render worthless the test for both tensile strength and elongation.

With a little experience the quality of a plate may be determined to some extent by breaking the corner off over an anvil and by inspecting the punchings from the plates. If the iron is brittle and untrustworthy, the punchings will show cracks in all directions if the punch be working as ordinarily with a little clearance; whereas if the iron is good and reliable, slight cracks only will be perceptible, all running in the direction of the fiber. While these workshop tests can be carried out in the manufacturer's yard by the inspector during the progress of the work, all tests requiring to be made with hydraulic presses or steelyards should be conducted by independent authorities, such as Mr Kirkaldy. After the material has been tested and passed and the structure put together, it becomes necessary to apply a proof load, which consists of gradually placing on the structure a weight somewhat exceeding its working load. This is requisite in order to ascertain if the workmanship is up to the proper standard. It must, however, be always remembered that a proof load is no test of the strength of the structure or the quality of the material. If the iron is hard and brittle it will give less than a material of more desirable quality, and the structure will apparently be stronger, but it is needless to state that such is not the case.

Again, any part may be on the point of breaking, and yet not yield sufficiently to materially alter the deflection. Likewise, although a structure may stand the application of a proof load at the time of testing, it does not follow that it will stand repeated applications of loads of even less amount than the proof load. Fairbairn's experiments carried out many years back demonstrated this fact. He found that when the strain on the iron of a beam was between 6 and 7 tons per square inch, the beam sustained an unlimited number of applications of the load producing this strain; but when the load was increased so as to put a strain of from 8 to 9 tons per square inch on the iron, the beam failed with a comparatively few applications of the load.

There is a variety of other matters of detail to be considered in designing and constructing ironwork, but time will not permit of their being now dealt with. The author will therefore only add in conclusion that the views herein expressed have previously been made public in a somewhat different form in the scientific press. This will account for any apparent similarity of ideas which may exist in this paper and the articles, which were, of course, unauthenticated.

SPEED OF ICE BOATS.

WE give below a number of letters showing how and why ice boats sail faster than the wind that propels them. A perusal of these letters will give the reader a good idea of the philosophic principles involved:

To the Editor of the Scientific American:

I desire to add to your comments on the discussion relative to "speed of ice yachts" a demonstration which, I think, will show that the problem of a "vessel sailing close to the wind" has scarcely been considered by those who deny the fact that a vessel may sail faster than the wind which propels it.

The wind being exactly abeam of a vessel, and her sails at an angle of 45° with her course (and consequently at same angle with direction of wind), would, without friction, travel exactly the same distance as the vessel.
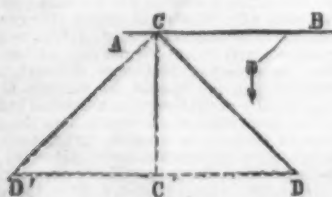
A vessel, A B, with sail extended, C D, would receive a pressure upon the sail tending to drive it in direction C D'. The resultant of this force would be a force striving to impel the vessel in direction C C', resisted by keel, centerboard, or runners, and the force C' D', tending to

advance the boat upon her course. In other words, we may imagine C C' D as a wedge, with point at D, and force attacking it at arrow, throwing it out of an imaginary socket. Now, if we sheet in the sail, C D, and let the wind remain
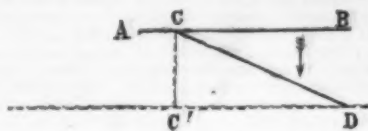
abeam by keeping her course unaltered, is it not easily perceived that, while the wind passes from C to C', the boat will pass a distance sufficient to allow point D to reach C', or our imaginary wedge, C C' D, will again be driven out of its socket a distance, C' D, requiring a movement on part of power so applied from C to C'? The impelling power, however, decreases in force as the sail is hauled aboard, while the component which, uncontrolled, would result in leeway increases in inverse proportion, so that, practically, at its friction at its minimum and leeway at its minimum, as in an ice boat, would, doubtless, lead to just the results practically claimed for it, and I do think your correspondents, scientific or otherwise, can appreciate the demonstration here given.

H. R. BAMHURST.

Erie, Pa.

MAXIMUM VELOCITY OF ICE BOATS.

To the Editor of the Scientific American:

In a recent issue of the SCIENTIFIC AMERICAN you discussed the ice boat problem, and it seems that it is regarded by some as theoretically impossible for an ice boat to sail faster than the wind which propels it.

On the contrary, the following demonstration will show that it is theoretically possible for an ice boat to sail with any velocity whatever without regard to the velocity of the wind:

To find the maximum velocity of ice boats, we assume a theoretically perfect ice boat, that is one so constructed that—

1. There is no friction between the boat and the ice.
2. The body of the boat offers no resistance to the wind.
3. There is no friction between the sail or sails and the wind (with such a sail the wind could only exert a pressure normal to the surface of the sail).
4. The runners will not slip on the ice.

For the sake of simplicity, the ice boat is assumed to have one sail, which is a perfectly rigid, plane surface.

Lemma.—The theoretical ice boat can be propelled in any direction except in the direction exactly opposite to the direction of the wind.

Dem.—Let A B (Fig. 1) represent the direction of the

FIG. 1.

wind, C D the position of the boat, making with A B the angle A B C. Let the sail, E F, be set in any position, so that the angle A B E < A B C.

From the principles of mechanics we know that the wind can have no effect on the sail, E F, except to exert a pressure in the direction G B, at right angles with the surface of the sail; also that the effect of the pressure, G B, on the boat, C D, is the same as the combined effect of a push in the direction S B, at right angles with C D, and a push in the direction D C. The first push would have no effect on the boat, and the second would move it in the direction D C.

Since the above reasoning will apply whatever may be the angle A B C, the truth of the proposition is evident.

Theorem.—The theoretical ice boat can be propelled with any velocity whatever, by any wind.

Dem.—It is evident that the theorem is true if at any assignable velocity of the ice boat the wind can still exert a propelling force against the boat. Let A B (Fig. 2) repre-
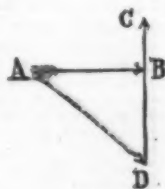

FIG. 2.

sent the direction of the wind; B C, the direction in which the ice boat is sailing. Let the distance A B represent the velocity of the wind, and (prolonging C B) let B D represent the velocity of the ice boat. It is evident that the direction in which the wind strikes the boat, *when moving under these conditions*, is the direction, A D. It is obvious from the previous demonstration that any wind, having the direction A B, could exert a propelling force on an ice boat at rest and ready to move in the direction B C, and it is plain that the propelling effect, when the wind strikes the boat in the same direction and with the same velocity, is the same, whether the boat is in motion or at rest. Moreover, the same reasoning will apply, whatever the assigned velocity of the ice boat, as compared to the velocity of the wind. Hence it follows that the theoretical ice boat can sail with any assignable velocity, whatever may be the velocity of the wind.

Cor.—From the demonstration it is evident that the angle A B C may be any angle except 180°.

Remarks.—The above demonstration shows that the perfect ice boat could sail with a velocity infinitely great, however small may be the velocity of the wind, except when the velocity of the wind = 0, or there is no wind. When this is realized, no one need be surprised if the best ice boat
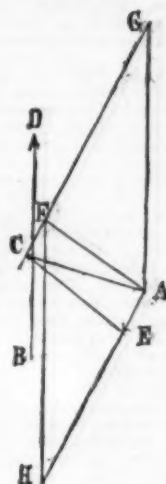
that can be constructed should attain a velocity many times the propelling wind when the conditions are favorable.

Q. E. D.

Boston.

To the Editor of the Scientific American:

That ice boats may reach a velocity greater than that of the wind which propels it may be shown, graphically, as follows: Let A C represent the direction and velocity of the

wind acting on the sail, C F, and moving the boat in the direction, B D; the force represented by A C can always be resolved into two forces, A E (acting parallel with the sail), and A F (acting perpendicularly to the sail). Resolve the latter into the forces, A G (in the direction of the motion of the boat), and A H (parallel with the sail), and we have the original force, A C, resolved into the three forces, A E, A H, and A G, the first two, acting parallel with the sail, would produce no motion in the boat; but A G, acting in the direction of the motion of the boat, produces the motion, and may be greater than the original force, A C, and, as it is a constant force, the motion will continue to increase until the resistance to motion becomes equal to the force, A G, which propels it.

G. M. R.

Leadville, Colo., Dec. 15, 1879.

To the Editor of the Scientific American:

The following simple discussion of the problem, "Can an ice boat sail faster than the wind which propels it?" may not be unacceptable to you.

For the sake of simplicity we will restrict our investigation to the case where the wind is on the beam.

Let α = angle C G B of the sail with course.
" β = angle E D E.
" $\theta = \alpha + \beta$ = angle F D H.
" v = velocity of wind, in direction ED.
" av = velocity of boat, in direction B A.
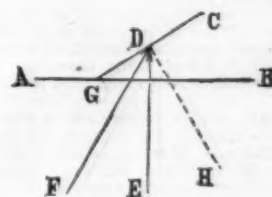
The question is, Can a ever be greater than 1?

To a person on the boat and moving with it, the wind appears to come from a direction F D, such that

$$\tan \beta = \frac{av}{v} = a$$

and its velocity is $v\sqrt{1+a^2}$

It is evident that the pressure of the wind is some function of the velocity—that is, P = $f(v)$ = pressure of wind per unit of area on sail.

For our purpose the form of the function is immaterial. The pressure resolved normal to the sail is

$$N = f(v\sqrt{1+a^2}) \cos \theta.$$

And this again resolved in the direction of the course is F = N sin. α, and substituting in this the value of N,

$$F = f(v\sqrt{1+a^2}) \cos \theta \sin \alpha,$$

or since $\theta = \alpha + \beta$,

$$F = f(v\sqrt{1+a^2})(\cos \alpha \cos \beta - \sin \alpha \sin \beta) \sin \alpha.$$

But

$$\sqrt{1+a^2} = \sqrt{1+\frac{\sin^2\beta}{\cos^2\beta}} = \frac{1}{\cos\beta}\sqrt{\sin^2\beta+\cos^2\beta} = \frac{1}{\cos\beta}$$

and we may write

$$F = \frac{f(v\sqrt{1+a^2})}{\sqrt{1+a^2}}(\cos \alpha - a \sin \alpha) \sin \alpha.$$

Hence the boat is acted upon by a constant accelerative force, F, and, provided there is no friction, will increase its velocity until F = 0; but F = 0 when a = cot. α.

Cot. α = 1 when α = 45°, and $a > 1$ when $\alpha < 45°$.

Hence we conclude that the boat will sail faster than the wind which propels it when the angle which the sail makes with the course is less than 45°, the wind being abeam and friction disregarded.

The friction in the case of an ice boat we know to be very slight, and as we have many well authenticated records of ice boats sailing at the rate of 60 or 70 miles an hour, while winds of such velocity are very uncommon, we may easily conceive that the friction is not great enough to materially

reduce the velocity below its theoretical value on the assumption that friction equals zero.

We observe that a increases as α diminishes, but when α is very small the component force in the direction of the boat's motion is very small, so that a limit of maximum speed is attained as α is diminished, until a further diminution lessens the velocity.

J. E. KEELER.

Johns Hopkins University, Baltimore.

WHY ICE BOATS SAIL FASTER THAN THE WIND.

To the Editor of the Scientific American:

That these boats do move faster than the velocity of the wind propelling them is a settled fact, and the philosophical principle seems to stand out very plain to view. Inclosed I send you a sketch, showing how these boats are driven faster by the wind than the wind itself moves. A B (dotted lines) show the course of the ship, A C show the position of

the sail, while the arrow shows the direction of the wind.

Theoretically the sail should travel the distance from A to B, while the wind is moving from B to C, which in the cut is about as one to five. But the rough cloth sail causes friction to the wind passing it, and it is impossible for a boat to run even on ice without friction. Hence there must be allowance made for this loss.

Were the sail a sheet of burnished silver, and the runners of the best possible form to overcome friction, no doubt that such a boat could be made to run two or three times the velocity of the wind which moves it.

P. H. W.

Sandy Hill.

To the Editor of the Scientific American:

To say that an ice boat may move faster than the wind that drives it, when the wind is *astern*, is equivalent to saying that the ice boat may move ahead in a dead calm. I speak of a steady breeze.

An ice boat may, and frequently does, move faster than the wind that drives it, but only when the wind is on the *beam*. When the courses of breeze and boat are at right angles to each other, they move with equal speed if the sail is set at an angle of 45° to the wind; but by setting the sail at an angle of 60° to the course of the wind the speed of the boat is greater than that of the wind. The reason is obvious. For convenience I have taken no account of friction.
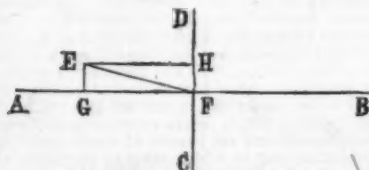
H. J. THOMAS.

New York City.

To the Editor of the Scientific American:

The ice boat question has evolved a good deal of nonsense. An ice boat cannot sail away from the wind that is driving it, but it may sail much faster than the air moves that drives it, because it sails in a different direction.

Suppose an ice boat to sail from A to B, the wind blowing

from C to D. Then the boat might, with its sail set in the direction E F, sail the distance G F, while the wind moved the distance G E, if there were no resistance; and, as the ice boat makes no leeway and has but little resistance, great speed can be attained with little wind.

The sailing of a ship with the wind abeam involves precisely the same principles, but the leeway and the resistance of the water are so great that the angle E F G needs to be about one-half at least that of D F G, in order to develop power enough to propel the vessel.

Good sailors will run within about "three points" of the wind, i.e., make the angle D F G about 35°, and E F G 15° to 20°. The absolute limit of speed of boat is to the speed of the wind as E H is to E G; and this remains true whatever the angles or proportion of the parallelogram E H G F.

Ice boats may approach this limit, but water boats fall far short of it for the reasons given.

LANDSMAN.

Binghamton, N. Y.

To the Editor of the Scientific American:

The writer of the article on page 381, volume 41, of SCIENTIFIC AMERICAN, "How Ice Boats sail faster than the Wind that drives them," evidently does not understand the true principle upon which ice boats frequently sail at a speed nearly double that of the wind which drives them. That a force acting continuously upon a movable body impels it with a constantly increasing velocity, I will admit, but only to a certain extent where there is an equilibrium between the impelling force and the resistance of the movable body caused by friction, consequently it is simply impossible that an ice boat sailing with the wind astern (where it will exert its greatest *force*) can equal, much less exceed, the speed of the wind which drives it. But if we can convert a portion of this force into speed by interposing some mechanical power, such as the inclined plane, then the paradox is explained.

The diagram will explain this. Let A B be the direction of the wind. The boat is free to move only in the direction D C. The sail is set at the angle, D B. Now while the wind moves from B to C, the boat will be driven from D to C, which gives a speed of three times that of the wind. Such a speed cannot be realized in practice because the force

(the wind) is a thin, elastic fluid, and the friction of the boat upon the ice, and the wind upon the sails, is considerable; but any one can easily understand how ice boats, sailing
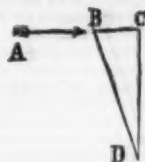
with the wind abeam or on the quarter, can attain a speed much greater than the wind that drives them, and upon sound mechanical principles.

A. F. BROCKWAY.

Barton Landing, Vt.

To the Editor of the Scientific American:

DEAR SIR: I have closely followed the controversy regarding the sailing of ice boats at a speed faster than that of the wind.

I have sailed ice boats of the better class at a very high speed, but never until last year have I taken the trouble to try and discover the cause of it. Last year, for the first time in ten years, the river at this point was in condition fit to use an ice boat, and I had one built in very rough manner, which cost me, outside of sail and rigging, only $2.75, and made a distance of 10 miles in 14 minutes over comparatively rough ice. The best time was made when close hauled, or with the wind on the quarter, and my explanation is as follows:

When an ice boat is running close hauled, it is, of course, running partially toward the direction from where the wind blows, and consequently the velocity of the boat adds to the velocity or force of the wind, and if there were no friction the velocity would increase *ad infinitum.*

All practical ice boat men will agree with me, that before the wind the speed of an ice boat is considerably less than the velocity of the wind, so much so that you can feel a current of air passing in the direction in which the boat is sailing.

E. H. SCHMIDT.

Davenport, Iowa.

THE VELOCITY OF ICE BOATS.

To the Editor of the Scientific American:

Several articles have recently appeared in this paper in regard to the paradox of a boat sailing faster than the wind which propels it, but while the facts are accepted, the explanations given are hardly satisfactory, if not absurd. The possibility of such a velocity under certain conditions is, however, susceptible of a ready and simple mathematical demonstration. In the first place it is impossible for a boat *sailing directly before the wind* to attain a velocity greater than that of the wind itself. The velocity in this case would be the same as that of the wind, leaving out of account the friction between the boat and the ice. For with this velocity the pressure behind and the resistance in front are the same, i. e., equal to zero. If this velocity is increased the resistance becomes greater than the propelling force, and brings it down again, while if the velocity is decreased, the force of the wind in turn overbalances the resistance, and so the equilibrium is maintained.

If, however, we suppose the wind to strike the boat in an oblique direction, the problem gives different results, and we here find the explanation of the apparent paradox. The force of the wind may be resolved into two components, one in the direction of the boat's motion, and one at right angles to it.

The first of these tends to give the boat a velocity equal to itself, as shown above in the case of a boat moving directly before the wind. The second component exerts a force on the sail which is constant and independent of the velocity of the boat. This force tends to give the boat a velocity such that the resistance of the air in front caused by this motion is equal to the constant force. These two components act entirely independently of each other. The velocity of the boat is equal to the sum of the velocities due to the two component forces, and in certain cases may become greater than the velocity of the wind.

The results of this reasoning may be summed up in a general formula as follows:

Boat's velocity = wind's velocity × (cos. θ + sin. θ cos. φ) in which θ and φ are the angles made by the line of the boat's motion with the direction of the wind and the plane of the sail respectively. This, of course, does not take into account the friction of the boat with the ice, but leaving that out, the formula holds in every case where φ is greater than zero.

This formula applies not to ice boats only, but is good in every case where a boat is propelled by the wind acting upon sails. But while in the case of the ice boat the friction is very slight, with an ordinary boat the friction and resistance of the water render such high velocities impossible. The value of θ corresponding to the greatest velocity is 45°, while the velocity increases with a decrease in the angle φ. But the reduction of φ is practically limited by the increase of a component which tends to careen the boat to one side.

FRED. K. SMITH.

Ann Arbor, Mich.

To the Editor of the Scientific American:

I have noticed with much interest the various articles on the subject of ice boat propulsion which have appeared in your columns of late, and I am surprised that nobody has given the very simple mathematical demonstration of the fact that an ice yacht can sail faster than the wind which propels it. I am the more surprised by reason of the fact that those two learned professors, like most theorists, would probably be much more easily convinced by a theoretical proof than by the direct testimony of what they might call "unscientific" witnesses to the fact.

In the accompanying diagram, let the line A B represent the length of the ice boat, and its direction represent the direction in which the boat is sailing, at right angles to the wind which is indicated by the arrows; and let B D represent the position of the mainsail. Now, when the boat has moved forward to the position, B C, shown by the dotted lines, the wind has moved through the distance, B E, while the boat has, in the same unit of time, moved through the distance, A B. The fact that the actual shape of the sail under pressure of the wind is not a plane, and also the use

of many sails of different shapes and sizes, would only modify these results.

If the sail was placed at an angle of 90° with the length of the boat, it would coincide with the direction of the wind,

and the boat would be motionless. At an angle of 45°, the speed of boat and wind would be (theoretically) equal, and as the angle became smaller the speed of the boat would increase until a limit would be reached, where the increasing friction of the boat, the bellying of the sail or sails, and other things would prevent any further increase of speed; and beyond that point the speed would rapidly decrease as the angle became smaller, until, when the direction of the sail coincided with that of the boat, it would again come to a standstill—very much in the condition of the Irishman's craft that had the misfortune to get "jammed betwixt two winds."

W. B. MANNING.

Colorado College, Colo. Springs, Colo.

IMPROVED DROP HAMMER.

A SOMEWHAT novel form of power hammer has recently been devised by Messrs. M. Hasse & Co., of Berlin. This tool, of which we annex illustrations, was brought first into public notice at the recent Berlin Exhibition, where it attracted considerable attention. Although presenting somewhat familiar general features, this hammer has several distinctive details, which are said to entirely remove the defects generally met with in drop hammers or stamps. The hammer head is attached to a broad wooden stem which passes between two rollers at the top of the machine, and the pressure upon the stem from these rollers is controlled by means of a lever, so that the stem can be held or released at will. It is in the form of this stem that one distinctive feature of the arrangement exists; instead of being of uniform thickness throughout, it is tapered as shown, the upper end being about ⅝ inch thicker than the lower. One advantage of this tapering stem consists in the ease with which the tup can be controlled, as a very slight pressure of the controlling lever is sufficient to exert sufficient grip of the roller to arrest the stem, even through the whole range of fall. In addition to this facility in stopping the tup in any position, it remains in that position until the rollers are released by the controlling lever. The slight pressure on the stem requisite for working the hammer involves also a very insignificant degree of wear, so that the stem is far more
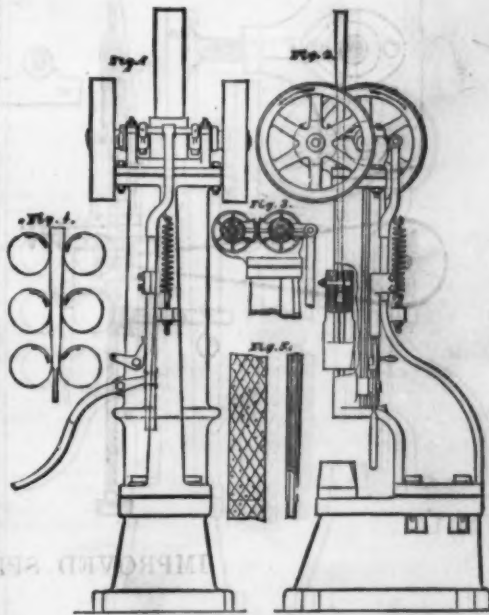
IMPROVED DROP HAMMER.

durable than the parallel ones hitherto employed. By a simple device the stem is prevented from rising too high and thus bringing the tup in contact with the friction rollers. For a length of about a foot at its lower part the stem is made parallel and its thickness is reduced, as shown in Fig. 5. By this means, if the stem is thrown up so rapidly that the lower and parallel portion passes between the rollers, the latter, not being able to advance closer together, cease to grip the stem, which falls until the shoulders upon it rest on the rollers, and these, when separated, allow the stem to fall between, as already described. The machine is driven by two belts, one open and one crossed, and the speed given to the friction rollers is about 120 revolutions per minute. Fig. 3 shows the arrangement for varying the position of the rollers by means of the lever, and Fig. 4 is a diagram of their relative positions at different points in the travel of the stem. Of course the system is applicable to any sort of framing, and for stamping purposes a double frame is generally employed.—*Engineering.*

OXIDATION PRODUCT OF CHOLIC ACID.—P. LATSCHINOFF. —An oxidizing impure cholic acid with potassium permanganate or nitric acid, the author obtains a mixture of crystallized acids, of which he here describes one, the choloidanic acid. No fixed fatty acids were obtained on a similar treatment of pure cholic acid. Choloidanic acid is isomeric with camphoric acid, but distinct from all the isomers of the latter hitherto described.

IMPROVED SPINNING FRAME.

IN the improved frame designed by Messrs. Newsome & Robertshaw, and which is applicable to wool, worsted, cotton, etc., the spindles and fliers are mounted in separate rails, so as to revolve independently of one another, the flier being driven by means of a band passed round a pulley formed on its boss, and the spindle following it at a reduced speed according to the amount of tension or drag which is put upon the yarn. This tension is regulated by means of a friction pulley, which is made to bear against the spindle with more or less pressure, according to the material under operation, this pressure being regulated by an adjustable weight.

The spindles employed are formed of metal tubing in the largest part, and solid at each end, which possess the required strength and elasticity, and are much lighter than solid spindles.

By means of this machine condenser threads can be wound without twisting them, or the amount of twist may be varied. The machine is therefore well adapted for winding condenser threads having a fine spun thread in their center. It is described as follows in the *Universal Engineer:*

Fig. 1 is a side elevation of the improved spinning apparatus, partly in sections; and Fig. 2 is a front elevation of the same.

Figs. 3, 4, and 5 are details of parts of the apparatus.

A are the spindles, which are constructed of steel tubing in the largest part, the foot and the neck being solid; B are the bobbins, which may be either fast or loose upon the spindles. The fliers, C, are mounted in a fixed rail, D, and have a collar, d, through which the spindle passes. The hole through the collar is larger in diameter than the spindle, and

PHOTOGRAPHS OF MICROSCOPIC OBJECTS.

THE object of writing this is to show by what simple means and ways a photographer with a microscope, and a microscopist with a camera, may obtain very satisfactory results in taking photo-micrographs or enlarged photographs of the "wonders of the microscope."

It seems that existing treatises on the subject, and the apparatus they describe as necessary, rather deter amateurs from trying their hand at this fascinating work. To produce the highest results for histological studies, it is indeed indispensable to have instruments of the highest power and all accessory apparatus for proper illumination; condensers, reflectors, heliostat, etc., together with a thorough knowledge of working the microscope, the extent and complexity of which most persons can have no idea. This, however, we must leave to the comparatively few select and privileged apostles and ardent disciples of science.

But if we cannot build a palace, we can perhaps build a little cottage, and live in it just as comfortably, though on a smaller scale.

Now, then, to business.

The photographer needs – besides what he has and knows – a good microscope that can be inclined horizontally, has a rack-and-pinion arrangement for focusing, and a concave mirror movable in any direction. When you have a microscope, any microscopist will be glad to make you acquainted with its use, if you do not know it already. How to apply it to the camera box afterward, I will tell you presently.

The microscopist needs—besides what he has and knows —a small, half-size camera box, having bellows for extension, a very fine ground glass for focusing, and a plate-holder perfectly adjusted in focus with the ground glass.

close to an open, sunny window, and alongside (not toward) the window, so that the sun can shine on the mirror of the microscope; draw the window shade down so as to shut off the sunlight above the mirror, and a pasteboard or thick sheet of paper may shade the camera, and especially the focusing glass. Everything is now ready to begin work, *secundum artem.*

Beginners will better use a low power objective, and a slide with a coarse object, such as a flea or other small insect (transparent, of course), first. Turn on the sunlight by the mirror until 'a full round circle appears on the ground glass of the camera. Pushing or sliding the camera toward the microscope enlarges the circle, and withdrawing it makes it smaller; but the eye-piece or tube must, of course, always be inside the camera. (The eye-piece is *not* taken out for our present work.) Now focus the object as usual by the adjustment screw of the microscope until a perfectly sharp image appears on the ground glass. Meanwhile you may have put a plate into the bath, which now will be ready for the plate-holder. Just before taking out the ground glass from the camera, look once more that the object is fully illuminated. Lose no time now, but place a piece of dark yellow paper between the slide and the objective glass of the microscope to shut off the light; take out the ground glass and put in the plate-holder, and raise the shield. Do all this with gentle motions. Now lift the yellow paper from the object for three or four seconds only, then replace it again and shut down the shield of the plate-holder. Carry into the dark closet, and develop with a rather weak (say 20-grain) iron developer. You will, of course, see at once whether the time of exposing in the camera was too long or not long enough, or just right, and govern yourself accordingly at the next trial. In this respect the photographer has the decided advantage over the microscopist. More time, even ten or fifteen seconds, must be given (owing to feebler lights) when a high power is used, or the camera extended by the bellows to fill up the whole plate instead of keeping the circle of light within it. The greater or lesser transparency of the object is also a matter for consideration.

The resulting negative may then be used for printing photographs on paper or for making glass positives for the lantern in the usual way. Of course it can be enlarged or reduced to any desired size by copying.

In conclusion, I would say that in the preceding directions I have endeavored to give in plain language, avoiding as far as possible all technical or scientific terms and phrases, explicit instructions for making photo-micrographs (not micro-photographs) by the simplest means. It is, of course, impossible in an article like this to instruct the photographer in microscopy and the microscopist in photography; this they must learn of each other. But each will have no difficulty in producing good results after some practical knowledge and experience. I may only remark, in conclusion:

First. That the morning sun and a cloudless sky are most desirable.

Second. The building, as well as the table or stand, must be absolutely free from vibration during the few seconds of exposure; a wagon passing by in the street shakes the whole building, even when of brick or stone. No walking in the room, of course.

Third. Absolute cleanliness and careful manipulation are indispensable in both branches of the art.

ON THE PREPARATION OF GELATINE EMULSION WITH GLYCERINE.

[A communication to the Photographic Society of Great Britain.

IT is now more than six months since I described the preparation of gelatine emulsion without washing the emulsion itself, but by washing the silver salt, precipitated from an aqueous solution, and in the interval we have been favored with various criticisms on the method, many describing it as—if I may call it so—a "bogus" method, and others writing of their success. To the first class I would simply say it is somewhat ungenerous to imply that something was described which could not be carried into practice; and if there be anything which deters experimenters from giving the non-experimenters the benefit of their knowledge it is the unhesitating way in which some of the latter class are ready to complain of non-success, which is usually caused by want of skill and thought on their own part. A relation of my own was the inventor of glue moulds for taking casts. He submitted a cast of a subject (and which was beyond the ordinary reach of a plastic mould) to the Society of Arts, and informed them that the cast was taken by means of a glue mould. The Society thanked him for the cast, but said the means of producing it he employed was *impossible*. A cast of a new subject and the glue mould used, after much debate, settled the point, and the gold medal of the Society was awarded him. The same spirit which existed then seems to exist yet. People seem to think that it is quite unnecessary to carry out minute directions when trying a formula. For instance: I know that many failures have occurred through dropping the silver into the soluble bromide instead of *vice versa*, and I have no doubt that other failures have occurred through similar mistakes. It is seldom that an experimentalist furnishes an account of his failures—I don't—and it might be presumed that failures always do happen in any new set of experiments.

I have endeavored still further to simplify the formula I have given, which perhaps will meet with greater favor at the hands of some, though probably we shall have accounts of failures.

I leave at present the question of the proper proportion of gelatine to the bromide; that is a point which I shall leave to the followers of Kennett, Bennett, etc., to settle. Suppose you want to emulsify twenty grains of zinc bromide, converting it fully into silver bromide. Take thirty grains of silver nitrate and dissolve it in one ounce of water, add one drachm of pure glycerine to it, and mind that the mixture of glycerine with the water is perfect. Dissolve the zinc bromide, twenty grains, in four ounces of water, and add this drop by drop to the silver solution, *stirring well*. When all is added the silver bromide will be found to be precipitated as a curdy mass at the bottom of the jar or vessel, with a slight milkiness with the supernatant fluid. This will subside in a short time, when the washing can be commenced as already directed in the above formula. Should there be an excess of silver, the first wash water should consist of four ounces of water to which two drachms of nitric acid have been added. The silver salt should soak in this for a quarter of an hour. The washing must be continued till the wash water shows no signs of acidity; the silver bromide is then drained as close as possible and turned into a bottle containing the requisite amount of gelatine dissolved in half the quantity of water it is intended to use. The bottle and

IMPROVED SPINNING FRAME.

is provided with a small bush, *e*, at the top, which fits round the spindle and steadies it without much friction. E are the rovings or condenser threads which are wound upon the bobbins, F, in the usual manner, and delivered therefrom by the surface drum, G. The roving passes between the rollers H, through the twisting tube, J, which puts in sufficient twist to enable the yarn to be drawn out by passing between other rollers, K, which revolve faster than the roller, H, from whence it passes through the guide board, L, to the flier and bobbin, B.

The roving is wound upon the bobbin, and receives more or less twist during the process, according to the speed of the flier, and the speed of the spindles is regulated by means of the brake, M, which is a grooved pulley carried on the end of a lever, M¹, mounted on a fulcrum, *m*, and having a balance weight, *n*, which presses the pulley against the spindle with a force varying according to its position upon the lever. For steadying the spindles, and preventing the brake, M, from pressing too heavily against the bush, *e*, a small tube, *p*, is employed, which is adjustable in a fixing, *s*, attached to the rail, D.

In Fig. 5 is shown a section of the hollow spindle, and a form of brake slightly different from that just described, the pulley being replaced by a short tube, S, of metal, fitting over the spindle, and attached to the lever, M³.

The following arrangement is employed for stopping the fliers: A forked brake, H, having a flat end, O, can be operated by the knee of the attendant, when the fork is caused to clip round the collar of the flier, and stops the motion of the latter. By this means both hands are left at liberty for joining "ends," or other purposes.

This brake is shown in plan in Fig. 4.

(This is most important.) No photographic lens or objective is required. Any photographer (they are, fortunately, more plenty than microscopists) will make you acquainted with the "dark art," get all the necessary materials, chemicals, and vessels, and arrange a dark closet for you. (How happy the microscopist of the future, when bath and collodion will be things of the past, and dry plates only known!)

Now let us suppose that both the photographer and the microscopist are provided with the working tools and materials as above stated, and have become somewhat expert in their mutual arts.

Get a nice, smooth board about four feet long, and three inches wider than your camera box; fasten cleats or strips of wood along both lengths of the board for the camera to slide within them firmly but smoothly.

Stand your microscope near one end and in the middle of the board; incline the tube horizontally, with the eye-piece end toward the camera; push the camera toward it until the microscope enters the round hole at its center. If much lower than the center, raise the microscope by an additional piece of board. If much higher, plane off a portion of the board to sink the microscope lower. After the microscope and the camera are thus well centered, fasten the foot of the microscope firmly then by cleats or otherwise. Next, close up the round hole in the camera with thick cloth or felt, leaving or making an opening in the middle, only just large enough to admit the microscope tube, so that no light between can pass into the camera; an additional strip of cloth around the tube may be necessary to shut out every ray of light.

Now, the camera and microscope being well "wedded" together, place the whole board upon a firm table standing

its contents are then well shaken up and put in a saucepan of hot water—boiling, if you like—and after five minutes the emulsion is shaken up till in a perfect froth. It is again put into the hot water and treated in the same manner after a lapse of another five minutes. Three of these shakings should perfectly incorporate the emulsion. An ordinary American egg beater can be used to make the froth, with a saving of time.

Plates coated with emulsion thus prepared, without further cooking, are very rapid and in a proper molecular condition. I am afraid to say how rapid they are for fear of being misunderstood.

Before concluding this short paper I wish to refer to the addition of ammonia to the emulsion, as recommended by Dr. Van Monckhoven. Many are not aware that silver bromide absorbs ammonia (NH₃) rapidly and with the same energy exhibited by silver chloride, and the sensitiveness of this compound is by no means the same as that of the silver bromide per se.

I have recently been trying bromo-chloride gelatine emulsion, as I believe others have also. I find that they are most satisfactory, and can be developed in light which would ruin an ordinary bromide plate. They are very rapid, and take a good deal of intensity. They are orange by transmitted light, inclining to a gray tint. It seems to me that the addition of chloride or iodide will help photographers in making certain of unveiled plates. Some two years ago I tried gelatino-chloride alone. I have repeated my experiments, and by means of ferrous oxalate have developed fair images on plates made with it. It is exquisitely sensitive, but the easy reduction of the chloride to the metallic state by any alkaline or iron developer renders it almost impossible at present to take advantage of it, except in a mixture of silver bromide. Mr. H. B. Berkeley, I believe, was the first successful worker in this direction.

For studio work, if you can obtain plates developable in a decent orange light, it seems to me that the present bromide plates will be out of the field. For outdoor work of the ordinary kind I still think that collodion emulsion carries the palm, and on another occasion I shall bring forward a collodion process which will press very hard on the heels of gelatine for rapidity.

W. DE W. ABNEY, F.R.S.,
Captain, R.E.

ELECTRIC GAS-PRESSURE REGULATORS.

AMONG the useful industrial applications of electricity, one of the most important is that relating to the regulation of the pressure of gas in distributing mains. The subject has been studied for a long time, and many systems have been devised, but up to the present time they have had but a limited use. The reason is that the apparatus hitherto devised has been subject to the control of the gas companies; and as these sorts of corporations have no interests in common with the consumer, and as the use of the apparatus tended to lessen the consumption of gas, there was but little chance of their finding favor and of being adopted. It became necessary, therefore, for inventors to consult the interests of the consumers themselves, and several of them then became possessed of the idea of combining certain devices, which, while attaining the same results, are especially designed for direct application to service pipes. Among such systems we may designate, as two of the best, those of M. Launay and MM. Chardin & Prayer. The first of these, represented in Fig. 1, is a signal system, and one that is so simple and practical that it has been adopted in many places. It consists of an alarm gong, which is put in operation, under the influence of a specific pressure of gas, by a battery, which becomes charged only at the very instant that such pressure has reached the degree desired. This pressure may, moreover, be very easily regulated by the consumer. The battery used by M. Launay is a sulphate
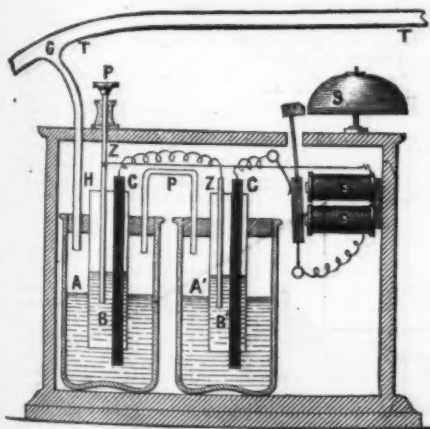

FIG. 1.—GAS PRESSURE REGULATOR.

of binoxide of mercury pile, composed of two elements, A A', united in tension, and each of them being composed of two vessels, one placed within the other so as to produce a hydrostatic pressure. For this purpose the larger of the two vessels, A A', which is a sort of recipient, is hermetically closed, and in this is contained the exciting solution; while the other vessel, B B', is a glass tube, open at each end, and into which dips a rod of carbon, C, and a pencil of zinc, Z. This tube passes through the cover of the recipient, to which it is properly luted, and is immersed in the exciting fluid. The carbon also dips into this liquid, but the zinc of one of the two elements, ending in a knob, slides through a hole in the upper part of the box which incloses the whole apparatus, and may, consequently, be placed at such a height above the surface of the liquid as necessary. The cover of the recipient contains two apertures, through which are introduced a rubber tube, G H, which communicates with the service pipe, where it leaves the meter, and a siphon, P, which forms a communication between the recipients of the two piles. With this arrangement it is easy to understand that, if the height of the zinc, Z, in the tube, is calculated so as to be 1 or 2 millimeters only above the liquid when the pressure is sufficient to give a proper supply of gas to the burners, it will take but a slight increase of such pressure to raise the liquid in the tubes and to cause the immersion of the zincs. The pile being thus charged, its current acts on the alarm gong,

which keeps ringing until the pressure has been removed by turning the cock of the meter. With M. Launay's apparatus it is possible not only to prevent accidents resulting from variations of the pressure of the gas in the tubes, but also to regulate the consumption in such a way as to obtain the very best effects possible at the very least expense. According to the inventor, the saving obtained is 25 to 30 per cent. This apparatus may also be used to ascertain leaks in the gas pipes in the daytime when all the burners are turned off. The gas having been turned on at the meter, the pressure soon causes the alarm to ring. The meter cock being closed, the bell will continue to ring indefinitely if there is no leak. But if there is an escape of gas anywhere, the gradually decreasing pressure will finally cause the alarm to cease its action.

Chardin's system, represented at Fig. 2, is an *automatic regulator* of gas pressure, and is applied, as we have stated, to the distributing pipes at the residence of the consumer. It is consequently composed of a transmitting or indicating apparatus, formed of a pressure gauge, and a regulating apparatus, which by the opening or closing of the valve
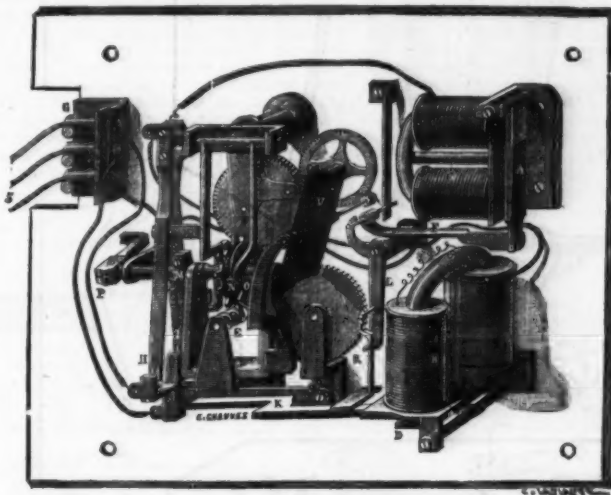

FIG. 2.—GAS PRESSURE REGULATOR.

under the influence of the first apparatus, reduces the flow of gas to the limits to which it should always be confined. The pressure gauge is an open, mercurial one, and is in communication with one of the gas pipes. A float is adapted to the surface of the mercurial column, and to this is attached a vertical rod whose upper extremity oscillates between two small platinum contacts. The distance between these contacts is so regulated that, if the float rises or falls a fraction of a millimeter, its rod encounters one or the other of the two; and the position of these is regulated so as to correspond with the limits of maximum and minimum pressure, which are not to be exceeded. By means of these contacts, then, two closings of the circuit can be effected, and which may act on the regulator in two different ways—that is, so as to close the valve or open it. The first effect is produced by a clockwork mechanism, actuated by an electro magnet, C, Fig. 2, which, through its armature, A, sets the mechanism in operation, with a resulting diminution in the outflow of the gas. The second effect is produced by another electromagnet, B, which reacts on the mechanism of the clockwork and winds it up, or, in other words, it causes it to work in a direction contrary to the first. This result is obtained by means of a ratchet wheel, R, which is fixed to one of the axes of the mechanism, and on which is mounted a clock and spring work, set in action by the armature, D, of the electro-magnet, B. This armature, moreover, is so arranged as to form a *vibrator*, as in an electric alarm. Now, as a result of this arrangement, so long as the circuit of the electromagnet, B, remains closed, the ratchet wheel, being set in action, revolves, and, while winding up the spring of the motive mechanism, effects by this very operation the opening of the valve until the flow of gas has reached a sufficient pressure to break the circuit of the electro-magnet, B, by suppressing the contact effected at the pressure gauge. In this system, then, there is no clockwork mechanism to be wound up, and all the functions of the apparatus are performed automatically by means of an electric current, requiring only a Leclanché battery of four or five elements. The following explanation of the plate will make the mechanism of the apparatus better understood:

C is the electro-magnet which starts the mechanism; A is its armature; F is the clockwork movement which this armature governs. When the latter is attracted, the pawl, L, is thrown back, the spring on the barrel uncoils, moving the endless screw, E, and the tappet, P, which is connected with the valve of the gas pipe. V is the fly which regulates the movement of the clockwork. B is the electro-magnet of the winding apparatus, and the armature, D, of which reacts on a ratchet wheel through the intermedium of the pawl, R, causing the mechanism to revolve in a direction contrary to that imparted by its barrel. K is the prolongation of the armature, D, which oscillates between the branches of a forked projection that serves to regulate the movements of the pawl, R. H I are contact springs, which serve to break the current when the clockwork movement reached the two extreme limits of its course in the two directions. M is a tappet which, moved by the clockwork, strikes the plate, I, and breaks its contact with the circuit. N is an arm sliding on the disk, O, which pushes it against the spring, H, and keeps it there till a notch in the disk comes in front of the spring, H, and breaks the contact by causing the latter to fall in the notch. 8 are the wires leading to the pressure gauge and battery.

This regulator is now being used with good results on the Orleans Railway.

RECOVERY OF TIN FROM TIN-PLATE SCRAPS.—According to a patent by Larocque, tin scraps are mixed with finely powdered charcoal and ½ per cent. of salt, and placed in a kettle that can be closed, supplied with a horizontal perforated diaphragm in the middle. The upper portion is then heated red-hot, whilst the lower is cooled with water, when the tin melts, and runs through.

POLARIZATION OF LIGHT.

FARADAY, in 1845, made the important discovery that bodies which (like glass or water) have ordinarily no particular action on polarized light, rotate the plane of polarization when influenced by strong electric or magnetic forces Many experimenters have since worked in this field, and it has been proved that not only in solids and liquids, but in gases also, an electro-magnetic rotation of the plane of polarization may be effected. MM. Kundt and Röntgen have recently described to the Munich Academy experiments by which they prove such rotation, even in the less easily condensed gases, hydrogen, oxygen, atmospheric air, carbonic acid, and marsh gas, and measure its amount. The method consisted essentially in compressing the gas strongly in a copper tube with glass ends, through which a beam of polarized lime-light was passed, the tube being surrounded by a coil of wire for passage of the current. For certain reasons the polarizer and analyzer were placed within or between the glass ends of the tube, and consisted of tourmaline plates. The polarizer end of the tube was fixed, while the rest of the tube, with the analyzer, could be rotated about its axis. The amount of rotation of the plane of polarization was found to differ considerably in the different gases, and to be greater the greater the index of refraction. In one and the same gas with varied density the amount of rotation is proportional to the density. An interesting question connected with such researches is that as to the possible influence of the earth's magnetism on polarization of light of the atmosphere. Some years ago, M. Henri Becquerel made a striking experiment, in which, a beam of light having been polarized by successive reflections through a glass tube containing sulphide of carbon, it was found that if the tube was in the plane of the magnetic meridian, the plane of polarization had not the same position when one looked northwards as when one looked southwards. But if the tube was at right angles to the magnetic meridian, the plane had the same position whether one looked east or west, and its position was midway between those in the two other cases. This seemed to indicate a magnetic rotation of the plane under the earth's influence. Recently M. Becquerel has made numerous observations on atmospheric polarization in regard to the earth's magnetism. The plane of this polarization should coincide with the plane of the sun (a plane, i.e., passing through the sun, the point looked at, and the observer's eye), when the sun's plane is vertical, if there were no disturbing influence. Now, looking at a point near the horizon, north or south, or, better, near the magnetic meridian, when the sun's plane is vertical, the plane of polarization is found by M. Becquerel to be deflected by a small angle, and the coincidence of the two planes never occurs till after passage of the sun. It is exactly as if the plane of polarization underwent a rotation, always in the same direction (which with reference to the axis of rotation of the earth is direct), and this rotation M. Becquerel attributes to the earth's magnetism. In the region at right angles to the magnetic needle the rotation was found to be *nil*.

ACTION OF SOLAR LIGHT UPON VEGETATION IN NORTHERN REGIONS.

NICOLAS DE NASAKINE.

The author maintains that the aroma of fruits increases with the latitude, while the sweetness decreases. The foliage of northern trees is remarkable for its deep shades of green, and the flowers have colors exceptionally vivid. Many herbs, such as carraway, are richer in essential oils in Norway than in more southern regions. The author ascribes these differences to the influence of the prolonged light of the summer months.

FIRE BRICK.

To the Editor of the Scientific American:

In the article on fire brick and terra cotta, published in the SUPPLEMENT of Dec. 27, 1879, there occurs a mistake. In the "terra cotta process" you print:

"The best lengths for firing are: for buff, from 1 minute 10 seconds, etc."

This should read:

The best lengths for firing are: for buff, from 1 foot 10 inches to 2 feet 4 inches, and for red, 1 foot 9 inches to 2 feet.

ANDREW MCCLEAN PARKER.

IMPROVED MANUFACTURE OF VARNISH.—Zingler has patented the following process in France: A mixture of 1 part of bi-sulphide of carbon, 1 of camphene or oil of turpentine, 1 of benzine or rectified petroleum, and 2 of woodspirit is heated in a closed vessel, and powdered copal or other gum is added in the proportions of 50 of the gum to 106 to 125 of the solvent. It is allowed to remain for several days, and is then decanted, and a fatty oil added.

SUGGESTIONS IN DECORATIVE ART.—MOSAICS IN THE CHANCEL, CORK CATHEDRAL.

MOSAIC WORK IN CHANCEL, CORK CATHEDRAL.

The artist of the designs on p. 3406 was Mr. H. W. Lonsdale; and the mosaics were executed by Messrs. W. H. Burke & Co., of Newman street, Oxford street, London, for the architect of the cathedral, Mr. W. Burges.—*Building News.*

THE BASSICK MINE, COLORADO.

The ground embraced by the Bassick Company consists of five claims, the Maine, Lookout, Spring, Georgia, and Triangle, aggregating 6,075 lineal feet by 300 feet, together with three mill sites of five acres each, and a town site of 160 acres. Of these claims the Maine, says the *Mining Record*, is the most notable for its developments, having been explored by a shaft in all 242 feet and by a tunnel running into the mountain at a depth of 200 feet below the shaft mouth. Since the mine was purchased by present company the shaft has been sunk forty feet, all in ore—the tellurides getting richer—making total depth of shaft in rich ore 282 feet.

This mine is the largest telluride vein as yet ever discovered. A fissure vein of unknown depth, length, width, and shape, filled with a concretionary mass of pebbles, bowlders, and fragments of adjoining country rock, all partially decomposed and impregnated, coated and cemented with tellurides, and sulphides of lead, zinc, iron, and copper—all rich in gold and silver. These pebbles and bowlders are intermixed with angular fragments, having evidently tumbled in from the surface of the adjacent country and the walls of the fissure itself, while the minerals came up from the depths below in the form of solutions, and were precipitated and deposited on the bowlders in the form of concentric rings or layers, in many places completely filling the interstices between them. Where the bowlders and pebbles are separated, the space is filled with fine clay and decomposed rock, quite rich in these minerals, and inclosing rich seams of tellurides and sulphides with wire and scale gold. In this mud and clay are embedded nodules of massive tellurides, weighing from one to forty pounds each. One of these measures two feet in length, eighteen inches wide, and six inches thick, and weighs 43 pounds. These nodules increase in size, richness, and number, and the bowlder scales and seams of mineral increase in thickness as depth is attained.

Although the extent of the ore body is unknown, yet it is certain from the report of engineers who have explored it that such an immense fissure has great length and depth, and the strongly marked, rich, and characteristic mineral formations contained within it undoubtedly stamp it as one of the richest and most extensive mineral deposits in the country. It is an exceptional mine, having been profitably worked from the day of its discovery without the investment of one dollar of capital; the yield for the first month was $12,000, which was subsequently increased each month that the mine was worked, until it amounted to the sum of $30,000 to $50,000 per month, while only sinking shafts and hoisting the ore, waste, and water, by man and mule power, and working only ten hours a day, while the mine occasionally stood entirely idle while running the tunnel.

Heretofore the ore was shipped to Denver, Pueblo, and Omaha, then sold to the smelters at the assay value less cost of transportation, and twenty dollars per ton for reduction, and ten per cent on gold, and fifteen per cent. on silver for loss in treatment. Now it is sold for cash to the Silver Cliff Smelting Companies at the assay value of the contained gold, silver, and copper, minus $1.25 for carting, and $20 per ton for reduction, and ten per cent. of the gold and silver for loss in treatment. This policy excludes the necessity of large capital being invested, and confines the production to the capacity of these works, and gives prompt returns. A forty horse power engine and hoisting machinery, with 500 feet of wire cables and necessary pumps, pipes, and other machinery for sinking the shaft to 500 feet are erected in the engine room. A boiler house, blacksmith shop, assay office, general office, and ore house, etc., have been erected at the mouth of the tunnel, and the mine is being vigorously developed. The cut opposite represents the underground developments which have been made on the property.

In further illustration of the high unquestionable value of the Bassick Company's property, we add the following extracts from the report of Professor Newberry:

"The Bassick mine is opened on what seems to be an enormous rift or fissure broken up through the trachytic rock, and subsequently filled with a loose mass of rolled and rounded pebbles and bowlders. The dimensions and direction of this fissure have not yet been fully ascertained, but it seems to have a general E. and W. bearing, and to be more than one hundred feet in width. Only one wall, if even that, has been reached in the underground workings, but the outcrops at the surface show that it is bounded by continuous walls of barren rock. The contents of this fissure, however, constitute the most remarkable feature in the mine; for instead of being filled with sheets of ore and vein stone, deposited in succession on the walls, as in the case with most fissure veins, this contains a mass of conglomerate consisting of pebbles and bowlders of trachyte, the interstices of which are filled with a silicious cement, and the various sulphides and tellurides which constitute the metalliferous portion. • • •

This ore body now forms the summit and principal mass of a hill some 200 feet in height, near the center of the original claim of Mr. Bassick, and it apparently reaches through a large part of this claim both east and west of the discovery. Trial pits sunk at several places east of the shaft have resulted in finding ore nearly to the eastern limit of the original location; and ore has also been found on the south-western end of the claim on the opposite side of the valley from the mine. Shafts and trial pits sunk on the Lookout and other claims belonging to the Bassick Company, lying contiguous to this on the south and east, have shown the presence of ore, but it has not yet been demonstrated that all these are parts of one ore body. The excavations in the mine have resulted in finding what seems to be one wall, though it is not certainly such; but barren trachyte is exposed on the surface west of the shaft, on the Bassick claim, and in excavations made on the Nemeha and other properties which bound the Bassick claim in that direction.

In the tunnel, which is driven in a northerly direction 320 feet, and strikes the shaft at a depth of 160 feet, most of the material excavated was called barren rock; but nearly all of it contained an appreciable quantity of gold and silver, and some good ore was found before the so-called wall was reached.

In regard to the extent in depth of the metalliferous deposit, it may be said that it has now been penetrated to the depth of 240 feet; that the ore at the bottom of the shaft is more abundant and richer than at the surface; and that all precedent and analogy point to the conclusion that it will be found to continue downward to the limit of profitable working. Every feature which the ore deposit exhibits indicates that it is the filling of a true fissure vein, and of the thousands of veins of this character which have been worked, the limit in depth of none has ever been reached. It is possible, and even probable, that the conglomerate of foreign material which has been introduced into the fissure from above, will be found to occupy but a portion of its depth, and below this the deposit may be expected to assume the character of an ordinary fissure vein. • • •

It may be said then that while the actual explorations of the deposit into which the Bassick mine is opened, have been carried but to a very limited extent, they have demonstrated the existence here of an ore body of very unusual dimensions, both laterally and longitudinally, and one that gives every promise of extending to a great depth. • • • •

As has been said, the metalliferous portion of the vein constitutes a considerable part of the cementing material which fills the interstices between the pebbles and bowlders. Sometimes it occupies all the interval between them, but oftener forms a shell or crust by which they are coated on every side except at their points of contact. This crust varies in thickness from one-quarter of an inch to a full inch or more, and is composed of layers of different kinds of ore which have some regularity in their arrangement, and seem to have been deposited at successive intervals of time. When the layers are most distinct, that next the surface of the pebble usually consists of argentiferous galena. Overlying this is a layer composed of hessite—the telluride of silver and gold—with sometimes interlacing wires of metallic gold and specks of iron pyrites. Outside of this is a thicker coating of the sulphides of copper and zinc, the latter predominating. The outer surface of the last coating usually shows distinct crystals, such as form on the walls of a cavity. Exterior to the crystallized surface of the outer coating of ore, and forming the filling of the larger spaces between the bowlders, we often find solid or cellular chalcedony—evidently a hot water deposit—and masses of kaolin, the result of the decomposition of the feldspathic porphyry or wall rocks. It should also be said that the silicious deposit and the clay always contain an appreciable quantity of silver and gold, often sufficient to justify the treatment as ores. • • • •

THE BASSICK MINE, ROSITA, CUSTER CO., COLORADO.

The percentage of silver and gold in the different varieties of ore as specified, varies much; for example, the argentiferous galena usually carries from eighty to a hundred ounces of silver, with from ten to twenty dollars in gold. The sulphides of zinc and copper contain a somewhat less percentage of silver but more gold, while the tellurides are the great repositories of both, when purest, having a value of from $4,000 to 7,000 per ton, most of which is gold. • • •

AMOUNT OF ORE EXTRACTED AND IN RESERVE.

. . . . Of the ore sold to the various reduction works at Pueblo, Denver, and Omaha, statements were rendered of the amount and value of the ore by those purchasing, and accompanied their checks for the ore. Many of these statements were preserved, and show that between July 21, 1877, and June 2, 1879, 731 tons of ore were sold, having an average assay value of $275.34 per ton, or a total value of $186,654.27, and yielded an average net value of $199.92 per ton=$145,144. Mr. Bassick claims to have taken out and sold more than double this amount during this period, and from June 2, to August 1, 1879, he claims to have taken out and sold more than $100,000 worth of ore. Besides these amounts there are over 4,000 tons of low grade ore left on the dumps, of an average assay value of $36.87 per ton= $147,497. All this was taken out while sinking the shafts and without stopping, the total excavation in the ore being about 115,600 cubic feet, which would give $3.65 per cubic foot, estimated from the actual returns shown and the value of the dump; and allowing sixteen cubic feet to constitute one ton, it would give a value of $58.40 for every ton of material excavated from the mine. At this rate of yield the value of the reserves now standing in the mine, without going beyond the limits already cut, would be fully $2,000,000. But the great value of the mine does not consist in simply stopping out these reserves, but in sinking down indefinitely on the vein, and taking out the richer and more compact ores that certainly exist there, as indicated by all the evidences of the mine.

Garcilasso de la Vego, writing in 1560, says that in the neighborhood of Arequipa and Arica, Peru, where it seldom rains, the natives manured their corn with fish, by putting two maize seeds into the head of one of them, which they then buried in a hole made with a dibble. The same observer states that in Peru, before 1560, turnips were almost considered weeds, and as such were rooted up, and spinach grew to an enormous height.

THE EFFECTS OF ALUMINA SALTS ON THE GASTRIC JUICE IN THE PROCESS OF DIGESTION.—EXPERIMENTS ON TWENTY-TWO LIVING ANIMALS.

(Read before the American Chemical Society, Jan. 8, 1880.)

By Henry A. Mott, Jr., Ph.D., E.M.

Alumina salts are introduced into the stomach in two ways—first by the adulteration of bread with alum, and second by the substitution of alum for cream of tartar in baking powders.

The introduction of alum into flour for various purposes has been a trick of the baker for the past one hundred years; fortunately its introduction is limited now to a few unscrupulous bakers—as in England, France, and Germany, it is an offense punishable by fine and imprisonment, to use alum in any connection with articles of food.

That alum is a poison, numerous experiments have demonstrated; and I believe it would be difficult to find a scientific man, who has properly investigated the subject, who would be willing to express any other opinion. The experiments conducted by Devergie and Orfila on living animals with alum (which must be admitted the surest method to arrive at the fact), clearly demonstrate its poisonous nature. They conclusively demonstrated that alum, in its hydrated and anhydrous (or calcined, exsiccated) condition, has a corrosive action on the mucous membrane; and, further, that it is sure to produce vomiting, constipation, extreme weakness, and loss of appetite, even in very small doses; and in such cases, if either by accident or intention, vomiting is prevented, death is sure to result.
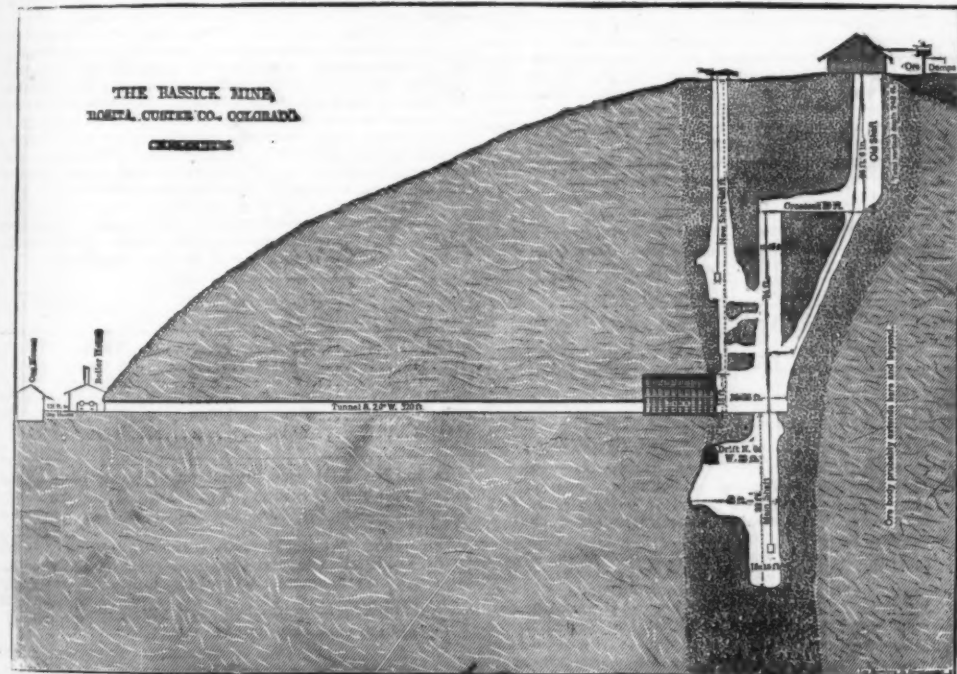
Several cases of poisoning by the accidental use of alum have been reported from time to time. A Mrs. B., reported by Dr. Fournier, "took by mistake a swallow of about three teaspoons of a solution of 16 grammes of calcined alum in a liter of water. Scarcely had she drunk it, when she pushed it away, complaining of nausea, severe heat, tearing pains in all parts in contact with the alum; her pulse had become rapid, and the face animated; the muscles had been agitated by slight convulsive movements; the desire to vomit had increased; the thirst had become inextinguishable."

Another case is reported by Dr. Ricquet, where a Mr. V. M., wishing to purge himself, ordered some sulphate of magnesia, but by mistake alum was given him; on dissolving 30 grammes in water and drinking the same, he succumbed after horrible pains. • "He felt a burning sensation in the mouth, throat, and stomach, followed by a sanguineous vomiting. No stools, extreme uneasiness, then insupportable anguish, repeated lipothymies, intelligence and sense intact; finally, intermittent filiform pulse, cold skin. The deglutition of liquid was almost impossible. He died eight hours after taking the poison."

Besides the last mentioned cause of death resulting from taking alum internally, must be mentioned three cases of alum poisoning by Von Hasselt, Taylor, and Hausemann, and two cases by Tardieu—all of which terminated fatally. Tardieu speaks of a woman who willfully murdered her three months old child by administering to it about 0·9 gramme (14 grains) of alum. In the face of such facts as these it is no wonder that there are laws against its use in articles of food.

When alum is used in flour in the manufacture of bread, some of the alum is decomposed by the phosphates of the flour and phosphate of alumina, basic sulphate of alumina, and some unaltered alum is left in the bread. From this fact much discussion has at various times arisen, but it has of course been entirely theoretical. In all my researches—it is certainly a notable fact—I was unable to find one scientific man who was willing to recommend the use of alum in bread making. On the contrary, I find the leading savants of the world, such as Dumas, Chevalier, Ure, Benoit, Gibbon, Schlossberger, Parkes, Booth, Morfit, Pereira, Normandy, the late Baron Liebig, and many others, denouncing its use in the strongest terms. All are agreed that the continued use of alumed bread will produce headache, indigestion, flatulence, constipation, diarrhea, dysentery, palpitation, and urinary calculi. I will simply quote from Dr. Gibbon, who says: "Its (alum) use in the manufacture of bread is injurious to health, and concurs ind.

rectly with other things in increasing the mortality, especially of young children, the staple article of whose dietary is bread. The fatal diarrhea of infants under three years of age may also have arisen from or have been aggravated by this cause." The introduction of alum as a substitute for cream of tartar in baking powders is a recent fraud on the public, and, strange to say, has been upheld by a few scientific men, who it would be supposed, would condemn its introduction in the strongest terms; but unfortunately for lack of time to properly investigate the subject, have been influenced away from the proper line of action, which should be not to see how near it is possible to poison a man in his food and not do it—but rather how far it is possible to keep from poisoning him. We have already seen that, when alum is used alone in bread, it is decomposed into phosphate of alumina, and that some alum is left unaltered. In an alum baking powder another chemical change takes place—owing to the presence of bicarbonate of soda—and hydrate of alumina is formed, a very soluble modification of alumina as compared with the phosphate or basic sulphate; but only just so much of the hydrate of alumina is formed as there is bicarbonate of soda present in the powder to form it; and as the alum is always in excess, being the acid of the compound, there is also formed phosphate of alumina, basic sulphate of alumina, and some alum is left unaltered.

From this it will be seen that just the same alumina salts are present in the baked product when an alum baking powder is used as when alum is used alone, with the addition of a far more soluble salt, hydrate of alumina, and this in larger quantities. It therefore naturally follows, as the simplest logical deduction, that whatever has been said against the use of alum, when used alone in bread, applies with all the greater force to an alum baking powder.

Because of the decomposition of the alum by the bicarbonate of soda, some scientific men have been induced to say that "there was no alum in the baked product" in which the powder was used; thus leading the public to believe, by a trick in wording, that the elements which compose the alum are driven off in the process of baking; while the truth of the matter is, that every element which composes the alum remains in the baked product, which, if eaten, enter the stomach, and are absorbed by the blood, acting the same as alum. This is not only my opinion, but it is the opinion of the leading scientific men of this country, such as Chandler, Barker, Johnson, Morton, Hays, Willard Parker, Alonzo Clark, Wm. A. Hammond, Ryland T. Brown, J. A. McCorkle, and J. H. Raymond, of the Brooklyn Board of Health, and many others.

Prof. S. W. Johnson, of Yale College, says:

March 21, 1879.

Alum and the soluble alumina salts are well known to be poison, which in small doses derange the digestive organs, and in larger quantities destroy life. I regard their introduction into baking powders as most dangerous to the public health. Bread made with baking powder containing alum must yield a soluble alumina salt with the gastric juice, and must therefore act as a poison.

I am decidedly of the opinion that the manufacture and sale of such baking powders ought to be interdicted with heavy penalties. S. W. JOHNSON,
Professor of Theoretical and Agricultural Chemistry, Yale College, Director of the Connecticut Agricultural Experiment Station, New Haven, Conn.

Some of the manufacturers of alum baking powders have induced a few scientific men to go a step further, besides saying that there is no alum in the baked product, but to actually get them to say that the alumina hydrate is rendered insoluble by the processes of baking, and that it would pass through the system like clay or any other inert substance.

This might seem plausible at first thought, but when we consider that it takes only ten to twenty minutes in a suitable oven to bake biscuit, and that after the biscuit is baked it contains itself about fifty per cent. of water, we see how utterly impossible it would be to drive off the water of combination of the hydrate of alumina, so as to render it insoluble as aluminic oxide.

It hardly seems necessary for any experiments on animals to decide a question of this nature so that the use of alum baking powders can be condemned, for a thorough scientific investigation of the subject can lead to no other conclusion. Still, as Prof. Patrick, of Kansas, conducted some elementary experiments on cats to sustain his position in stating that alum baking powders are not injurious to health, and as such experiments were interpreted by him favorably—although I hope to show, and am quite positive I will, that his experiments are most detrimental to his views, and most favorable to the side which condemns the use of alum baking powders—I thought it advisable to conduct an exhaustive series of experiments on dogs in search of the truth, believing that such an investigation would meet with the approbation of the public.

It was with difficulty I found a suitable place to conduct the experiments so that the animals would not disturb the neighborhood; but, through the courtesy of the Commissioners of the Dock Department, I secured a shed on their premises, foot of Sixteenth street and East River. This shed I had completely remodeled into a suitable house, having the dimensions of about 16x14x12 feet high. Sixteen stalls were made inside, having the dimensions of 3½ feet by 2 feet by 2½ feet. The bottom of each compartment was covered with straw, making a pleasant bed for the dogs. I then secured sixteen dogs from the pound, which were all carefully examined to see if they were in a perfect state of health. None but strong, healthy dogs were selected. The breed, age, food, color, and weight of every dog was carefully noted. Each dog was then consigned to a stall, and securely chained, and they all received a number, from one to sixteen. I commenced my experiments on the 9th of September, and finished December the 3d. My assistant was with the dogs from morning until night, and never left the animals without first securely bolting and locking the doghouse. No stranger was allowed to enter the house unaccompanied either by myself or by my assistant, and the dogs never received a mouthful of food or anything else from any one except my assistant and myself.

I will now detail the result of my experiments:

Dog No. 1—Breed of dog—coach; age of dog—1 year; health of dog—perfect; food of dog—bread and crackers; color of dog—spotted black and white; weight of dog—35 lb.

To this dog, on the morning of the 9th of September, was given 8 biscuits, at ten minutes past eight o'clock. The biscuits were made by myself as follows:

1 quart sifted flour; 20 teaspoons alum baking powder; 2 cups of water; 1 tablespoon of butter; 22 biscuits made, weighing 27 ounces; time of baking 20 minutes.

At half-past eleven, just three hours and twenty minutes,

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the dog was taken very sick, vomiting profusely—his vim and brightness of eye had departed, and he trembled considerably in his limbs.

At four o'clock, five more biscuits of the same nature were given, but he would not eat them.

The next morning eight more fresh biscuits were given him; he ate only a part of one. During the day previous he was quite loose in the bowels; but he had now become very constipated, and it was only with great effort and pain he was able to relieve himself for several days.

On September 11, as he would not eat the biscuits alone, they were mixed with meat; this he ate, but remained very dejected in spirits and extremely constipated.

To dog No. V. the same food was given. The description of the dog was as follows:

Breed—terrier; age—nine years; health—perfect; food—crackers; color—brindle; weight—30 lb.

At 8:15 on September 9, 8 biscuits, made and described above, were given. At 12:15 the dog became very sick and vomited profusely. At 4 P. M., five more biscuits were given him, but he would not eat. He was very constipated toward night. On the following morning eight biscuits were given him, which he ate in part during the day; in the afternoon he was very sick, vomiting at 4:30 and again at 5:45 P. M.

Experiments were next made, using only half the quantity used above of an alum baking powder.

The biscuits were made as follows: 1 quart sifted flour; 10 teaspoons alum baking powder; 1⅓ cups of water; 1 tablespoon of butter; 27 small biscuits, weighing 25½ oz.; time of baking, 11 minutes.

Three dogs were fed with biscuits thus made, with the following results:

	No. II.	No. IV.	No. VI.
Breed of dog	Cur	Spitz Cur	Shepherd.
Age of dog	15 months	1 year	4 years.
Health	Perfect.	Perfect	Perfect.
Food	Bread	Crackers	Crackers.
Color	Black	Yellow	White.
Weight	16 lb.	10 lb.	40 lb.

Eight biscuits were given to dogs Nos. II. and VI. in the morning; in the afternoon dog No. II. was very loose in his bowels, and dog No. VI. very constipated. Five more biscuits were given in the afternoon and eight more the following morning, part of which were eaten. Both the dogs then were extremely constipated and apparently quite sick, although they did not vomit. To-day dog No. IV., in perfect health, was then given three biscuits, which were eaten at nine o'clock. At 10:35 A. M., the dog became quite sick and vomited. In the afternoon and next morning more biscuits were given him, but he would not eat.

This demonstrates that some animals are more susceptible to the action of poisonous substances than others.

It now became necessary to know if the same effects would not be brought about by using the same quantities of cream of tartar powder. I therefore conducted a series of experiments to arrive at this point. Three dogs were experimented on. The following is a description of the animals:

	No. IX.	No. X.	No. XVII.
Breed of dog	Mongrel	Mongrel	Terrier.
Age of dog	4 years	10 years	2 years.
Health	Perfect	Perfect	Perfect.
Color	Blk. and white	Blk. and white	Blk. and tan.
Weight	20 lb.	35 lb.	15 lb.

The biscuits were composed as follows: 1 quart sifted flour; 20 teaspoonfuls cream of tartar baking powder; 2 cups of water; 1 tablespoon butter; 20 minutes baking; 26 small biscuits; weight, 27 oz.

The biscuits given to dog No. IX. were twice as large—only 12 being made instead of 26, therefore each dog was given as many biscuits as he would eat—without in any way affecting them. Their bowels were not in the least affected. Each dog ate sixteen biscuits the first day, eight in the morning and eight at night. Dog No. X. did not eat but ten biscuits; the next day each dog ate the biscuits again with appetite. Dog No. XVII. was fed four days on the biscuits, and ate same with appetite, without showing any signs of sickness.

These experiments clearly demonstrate that the salts left in the biscuit when a cream of tartar baking powder is used, are perfectly harmless; but when an alum baking powder is used are very dangerous, as in every case where dogs were fed on biscuits made with such powders, the dogs were made very sick, causing them to vomit profusely, lose all energy, and show weakness in their limbs.

The next series of experiments were to ascertain what effect would be produced by feeding dogs with hydrate of alumina mixed in with their food, as also phosphate of alumina. To two dogs, Nos. XV. and XVI., by drate of alumina was thus given.

The following is a description of the dogs:

	No. XV.	No. XVI.
Breed	Mongrel	Mongrel.
Age	1 year	3 years.
Health	Perfect	Perfect.
Food	Bread	Bread.
Color	White	White and black.
Weight	18 lb.	20 lb.

The hydrate of alumina was prepared by Professor Schedler; it was made by precipitating the alumina in alum by means of ammonia, and then thoroughly washing the same with water until the washings were perfectly free from traces of ammonia. The precipitate was then dried between blotting paper, and analyzed to ascertain the percentage of water it contained. The following is an analysis of the same:

Hydrate of alumina	12·48 per cent.
Abnormal water	87·52 "
	100·00 "

From this analysis it will be seen that 1 oz. of the precipitate is really only ⅛ oz. of hydrate of alumina or 54½ grains. To dog No. XVI. on the 13th of September, was given 1 oz. of precipitated hydrate of alumina (54½ grains.

Al_2O_3, $3H_2O$) mixed with meat at a quarter past eight in the morning. At 12.30 the dog became quite sick, and vomited; at ten minutes of six in the afternoon ⅛ oz (109·2 grains) more of hydrate of alumina in meat was given to the dog, and at twenty minutes past six he was again taken quite sick, and vomited; he vomited also considerable during the night, the meat being vomited up undigested. The next morning ⅛ oz. (109·92 grains) more of hydrate of alumina mixed with meat was given to the dog, and he vomited a short time afterwards; he was very constipated, his last stool being quite black. At three o'clock 109·2 grains more were given him, and he was again taken sick, vomiting, and showing great weakness in his limbs. The next day at three o' clock he was given ¼ oz. more of hydrate of alumina mixed with meat, when he was taken extremely sick, vomiting several times, and showing great weakness in his limbs and loss of ambition, the brightness of eye having disappeared; he vomited during the night, and could not be induced to eat any more the next day or the day following.

To dog No XV. was given ⅜ oz.(163½ grains) of hydrate of alumina mixed with meat. The dog was taken very sick in about two hours, and vomited just two hours and fifty minutes afterwards; he also vomited profusely during the night. At 4:30 the next day ½ oz. (218 grains) of hydrate of alumina mixed with meat was given the dog, but he ate only about one-half of it. He was taken very sick a short time afterwards, vomiting, and showing great weakness and restfulness. He would not eat any more after that day. It may be well to state here that hydrate of alumina is almost tasteless, and it was for this reason the dogs ate it as well as they did when mixed with meat. To two other dogs hydrate of alumina was given only once, and in each case the dogs were made sick, and vomited.

To dog No. IX. was given phosphate of alumina mixed with meat. The following is a description of the animal:

Breed of dog—mongrel; age—4 years; health—perfect; food—bread; color—black and white; weight, 20 lb.

On Sept. 18, in the morning, 3 oz. of precipitated phosphate of alumina (containing 75 per cent. of water, dried between blotting paper) was mixed with meat and given to the dog. This was eaten during the day, but the dog did not vomit, although he was evidently quite sick. The next morning 2 oz. more of the precipitated phosphate of alumina mixed with meat was given him, which was all eaten, and although the dog did not vomit, he was quite sick, showing less life than usual, and his eye not being as bright.

From this last experiment it was clearly shown that the alumina in biscuits made with an alum baking powder must be, to a very great extent, in the condition of hydrate of alumina—as the phosphate, although causing the animal to feel unwell, did not make him vomit. In every case, as has been stated before, when biscuits were given to a dog made with less than seven times the quantity of an alum baking powder usually employed, the dog vomited profusely, and was made very sick, trembling in his knees; and this was the case when hydrate of alumina was given, even in such small quantities as ¼ of an ounce, or 54½ grains. Experiments were then made to see if the action of hydrate of alumina in any way differed from the action of alum itself. The following is a description of the dogs employed:

	No. XIII.	No. XIV.
Breed	Terrier	Terrier.
Age	2 years	2 years.
Health	Perfect	Perfect.
Food	Bread	Bread.
Color	Black and Tan	Tan.
Weight	10 lb.	20 lb.

To dog No. XIII. was given 2 oz. of burnt potash alum mixed with meat at 8:15 in the morning. The dog ate only the meat, leaving the alum untouched, with the exception of what adhered to the meat, which was much less than ¼ of an ounce. At 9:30 he was very sick, trembling in his limbs, losing all vim and brightness of eye, and vomited. At 9:45 he vomited again. The next day some fresh meat was mixed in with the alum; when he ate part of the meat he was made very sick again, and vomited considerably. He would not eat any more after this.

To dog No. XIV. 1 oz. of ammoniac alum was mixed with meat and fed. At 8:15 only about ⅛ oz. was eaten. At 9:45 he was made very sick, the same as dog XIII., and vomited; he vomited again at 9:45, and again at 9:55, and was a very sick dog, showing no inclination to eat or play; his brightness of eye had entirely disappeared. To two other dogs alum was given with the same results. From these experiments it will be clearly seen that hydrate of alumina acts in the same manner as alum, causing the animal to vomit profusely, show great weakness in the limbs, and loss of ambition.

The next experiments conducted were to ascertain what effect the presence of alum, hydrate of alumina, phosphate of alumina, and basic sulphate of alumina had on the solvent power of the gastric juice. It was necessary, therefore, to procure some gastric juice for experiment. I therefore sent several dogs to Prof. J. W. S. Arnold, who inserted a canula in each of them. When the dogs were in a perfectly healthy condition, Prof. Arnold sent me some gastric juice, which was produced by tickling the lining of the stomach of the dogs with a feather or glass rod, which caused the gastric juice to flow out of the fistula into a receptacle placed underneath the dog to receive it. This and other methods were used to excite the flow of the secretion.

In conducting the experiments with the gastric juice, I was greatly assisted by the friendly services of Prof. Robt. Schedler. Four samples of gastric juice were received. The following are the experiments conducted with the same.

Sample No. 1.—Obtained by irritating the lining of the stomach with a glass tube—pure and free from food. The acid was determined in this sample, and found to be 0·1338° hydrochloric acid.

Sample No. 2.—Boiled ox heart was fed to the dog, which caused a flow of gastric juice, which was afterward drawn off. The acid in this sample was only 0·006083° hydrochloric acid.

Sample No. 3.—In three grammes of this juice the acid was determined and found to be 0·2126° hydrochloric acid.

Experiments were then made with this sample as follows:

To three grammes of juice was added 0·0408 grammes of fibrine,[*] and the mixture was kept at the temperature of 95

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[*] The fibrine was prepared by Prof. Arnold from the blood of a dog.

-100° F. for half an hour, when all the fibrine was dissolved.

To three grammes more of the juice were added 0·5 grammes of hydrate of alumina (precipitated and dried between blotting paper), and then 0·0403 grammes of fibrine was added. The mixture was stirred and kept at the temperature of 95–100° F. for two hours, and from 70–80° F. for twenty-three hours. Digestion of the fibrine took place at the start, but was soon arrested, only one quarter of the fibrine being dissolved.

To three grammes more of the juice were added 0·500 grammes of alum, and then 0·0403 grammes of fibrine, and this was treated the same as in the last experiment. In this case about three-quarters of the fibrine was dissolved at the start, and then further digestion was entirely checked, although it remained in contact twenty three-hours.

These three experiments are very valuable, as fibrine is so readily dissolved. They show that both alumina, hydrate and alum can check the digestion of such an easily digested substance as fibrine. They show, therefore, how dangerous it is to introduce these two salts into our stomachs, if we do not wish to excite indigestion and dyspepsia.

Three experiments were then conducted with prepared boiled white of egg. To three grammes of gastric juice were added 0·25 grammes of albumen, and the juice was kept at 95–100° for two hours, when half of the egg was dissolved. Three more grammes of juice was then added, when in two hours all the egg was dissolved. This showed that 100 grammes of gastric juice would dissolve 4·16 grammes of albumen. Lehmann claims it will dissolve 5 grammes, and Schmidt 3·95 grammes, although the latter authority states it may dissolve more.

To three grammes more of gastric juice were added 0·25 grammes precipitated hydrate of alumina (really only 0·031 grammes, $Al_2O_3,3H_2O$), and then 0·25 grammes albumen; the mixture was kept at the temperature of 95–100° F. for two hours, and in contact fifteen hours, and not a particle of the egg was dissolved.

To three grammes more of the same juice were added 0·25 grammes of alum, and then 0·25 grammes of albumen, and this was likewise treated; but after fifteen hours contact not a particle of the albumen was dissolved. These experiments were duplicated.

The albumen used in the experiments was the boiled white of egg: it was first macerated in a mortar with pure water, then dipped in a solution of 1 drop of hydrochloric acid to 2,400 drops of water; it was afterward macerated again in the mortar with pure water, then dried between filter paper and weighed.

The three first experiments demonstrate beyond a shadow of a doubt that both hydrate of alumina and alum check the digestive properties of the gastric juice, and render it incapable of digesting even the most digestible substances; and the last three experiments demonstrate that the digestive power of the gastric juice is entirely destroyed by hydrate of alumina and alum, so far as dissolving the more indigestible substances, such as the boiled white of egg.

The alumina renders the pepsin entirely inactive, probably converting it into a species of leather, and in the stomach the lining membrane and cells are probably thus affected, thereby destroyed or rendered incapable of performing their normal functions.

Experiments were next made with phosphate of alumina and basic sulphate of alumina.

To three grammes of a fresh sample of gastric juice were added 0·1 gramme of precipitated hydrate of alumina and 0·1 gramme of boiled white of egg.

To three grammes more of the gastric juice were added 0·1 gramme of precipitated hydrate of alumina and 0·1 gramme of boiled white of egg.

These two mixtures were kept between 95° F. and 100° F. for two hours, and in contact twenty-four hours, and not a particle of the albumen was dissolved in either case. These experiments were duplicated with fresh gastric juice from another dog, with the same results. These experiments show that all alumina salts interfere with the powers of digestion, having the property of rendering the pepsin inactive.

My next experiments were to ascertain whether alumina could be found in the various organs of the body if a dog was fed with hydrate of alumina. I therefore secured a dog from Prof. Arnold, of which the following is a description;

Breed of dog—terrier; color—black and tan; age—1½ years; weight—20 lb.

This dog had a gastric fistula through which the hydrate of alumina suspended in a water solution was introduced direct into the stomach by means of an ordinary syringe. On the 21st of October, at 8:30 A.M., 5 oz. of precipitated hydrate of alumina and 2 oz. of meat were mixed together and given to the dog. He ate only one-third of the mixture; at 11:35 his bowels were very loose, and at 12:40 he vomited; at 12:55 he vomited profusely again, the meat coming up undigested.

At 3 P.M. ¼ of an ounce of hydrate of alumina, suspended in solution, was injected directly into the stomach. The dog vomited during the night. The next morning, at 9:25 A.M., one ounce of hydrate of alumina was injected into the stomach and the dog was given meat to eat. He vomited at 1:30 P.M. and was very constipated; vomited at 2 P.M., and again at 2:15 P.M. At 3 o'clock, one ounce more of the hydrate of alumina was injected; at 5 P.M. he vomited; he also vomited during the night and was very constipated. At 8:45 the next morning about one ounce more of the hydrate of alumina was injected; he vomited at 11:45 and again at 12:55. At 4:55 P.M., ¼ ounce more of hydrate was injected, the dog vomiting during the night. The dog now was so completely under the influence of the hydrate of alumina that I fully believe he would have died if any more alumina was injected. He was a very sick dog, trembling in his knees when he stood up and wanting all ambition and vim. His eye was dull, all the brightness had departed. On the next morning, at 8 o'clock, I killed the dog, collected some of his blood, and took his liver for analysis. I separated from the blood by analysis a considerable quantity of alumina, as also from the liver. The silica and phosphate of lime were first removed before the alumina was precipitated.

My next experiment was on a black and tan dog in Prof. Arnold's laboratory. I supplied Prof. Arnold with freshly precipitated hydrate of alumina, and he fed the animal with the same for four days, when the dog was killed. I received the kidney, heart, and blood for analysis, in all of which I separated out alumina in large quantities. Professor Arnold examined the stomach and intestinal canal, as also analyzed the spleen and liver. His report is given below.

The next dog experimented on was also a black and tan. To this dog Prof. Arnold fed precipitated phosphate of alumina (containing 75 per cent. of water), mixed with meat. On killing the dog, I took the spleen and liver for analysis,

and separated out large quantities of alumina from them. Prof. Arnold examined the stomach, etc., and also analyzed the heart.

Report of Prof. J. W. S. Arnold:

UNIVERSITY OF THE CITY OF NEW YORK, }
MEDICAL DEPARTMENT, 410 EAST 26TH ST., }
NEW YORK, Dec. 12, 1879. }

This is to certify that I have supplied Dr. Henry A. Mott with a number of samples of gastric juice from the dog, the juice being pure and in the normal condition.

I have also made a number of gastric fistulæ in dogs; some of the animals I delivered to Dr. Mott, from others I obtained the juice with which I supplied him.

I fed a dog upon meat mixed with precipitated hydrate of alumina (containing much water); the amount of this hydrate of alumina given the dog was twelve ounces. I killed the animal, and examined the viscera. The duodenum was highly inflamed in its upper portion. The spleen and liver, upon analysis, showed the presence of a considerable quantity of alumina. The heart, kidneys, and samples of the blood from the animal were given to Dr. Mott for analysis.

I fed another dog with precipitated phosphate of alumina (containing much water) mixed with meat, to the amount of five ounces of this phosphate of alumina. Upon killing the animal, both the stomach and duodenum were found very much congested. Upon testing, the heart showed the presence of considerable alumina in its tissues.

Dr. Mott received portions of liver and spleen for analysis.

I also prepared microscopical slides of a dog's stomach in a healthy condition, and of the stomach of the dog fed with precipitated phosphate of alumina in a congested condition. These I sent to Dr. Mott.

J. W. ARNOLD, A.M., M.D.,
Prof. Physiology and Histology, Med.
Dep't University of New York

From these elaborate experiments it will be seen that both hydrate of alumina and phosphate of alumina are very injurious substances to introduce into the stomach, as these are sure to produce acute inflammation.

It may be advisable to say a few words with respect to some experiments conducted by Prof. Patrick, of Kansas, on cats, with an alum baking powder.

Some biscuits were made with 3 teaspoons of an alum baking powder to 1 pint of flour, 6 biscuits, equal to six teaspoons to one quart of flour, were baked in a batch, and from 11¼ were fed to to a cat. After digestion had gone on a certain length of time (from 20 minutes to 2½ hours), varying in the different subjects, the cat was killed, and the entire contents, not only of the stomach, but of the small intestines also were examined for dissolved alumina. The mass was digested in water, filtered, evaporated, and ignited to destroy organic matter; extracting with strong acids, filtering, and finally adding ammonia hydrate. "In every case," says Prof. Patrick, "a large amount of sodium sulphate was found (in solution, as was expected), and also a certain amount of hydrate of alumina undissolved." What the Professor means by "a certain amount of hydrate of alumina undissolved," it is difficult to ascertain. Surely if it was undissolved he might have dissolved it by the aid of a little heat and a little more acid.

The truth of the matter is, if the *filtered* solution contained any alumina, it was combined with the organic matter. On ignition the alumina would be rendered insoluble. If not insoluble, where did the insoluble alumina he obtained come from?

Perhaps he obtained it on the filter. This would clearly show that it was still in the stomach, not having been as yet absorbed. If this was not the case, and no alumina was found in solution in the digestive fluids, then the alumina must have been absorbed into the system, for it certainly entered the stomach through the biscuits.

Prof. Patrick further states: " Now, if bread is carelessly mixed with an insufficient amount of water, part of the flour (and with it the powder) remains nearly or quite dry; and, after baking, such bread would contain a certain small amount of alum." This is certainly a very fair admission. We all know that bread is very carelessly mixed at times, as there are few who make good bread. Patrick's experiments actually prove this to be the case. He says: " To insure the entire absence of alum in the bread, the mixing must be done with plenty of water; and to effect this I would suggest (although I do not consider it an absolute necessity) that the batter, with the powder added, be made rather thin at first, and then thickened by addition of more flour with out powder." In other words, Prof. Patrick would upset the whole system of bread making so as to insure the use of an alum baking powder with what he calls safety.

It is certain a few intelligent cooks might be persuaded to adopt this new method, but the majority could not be persuaded to do so; or, if they did, they would only do so once or twice, and then fall back on their old ways, which would result in having alum in the bread.

I think we can safely discard Prof. Patrick's experiments as proving anything in favor of alum baking powders, for in my opinion they only strengthen the view which I have always taken, and which my elaborate experiments have conclusively demonstrated, that alum baking powders are injurious to health.

It has never been asserted by me that a person eating one biscuit made with an alum baking powder would suffer from the alumina salts present in it; but it is certain that persons continually eating biscuits made with an alum powder will suffer from its poisonous effects, as the alumina salts, instead of passing out of the system, accumulate in the various organs, interfering with their proper functions.

I will close with presenting a letter to me from Prof. E. S. Wayne, of Cincinnati, in which he states that two families were poisoned by the use of alum baking powder:

CINCINNATI, April 10, 1879.

DEAR SIR: I have read your reports on baking powders with interest, and fully indorse all you say respecting them and their use.

I have met with two cases of poisoning here that could be traced to nothing else but alum baking powder. A Mr. Edwards, wife, and children were all made very sick by eating cakes made with it, and their symptoms were so similar to that of arsenical poison that they supposed they had been so poisoned. The case was handed to me, and I found nothing in either cakes and powder but alum.

So also with the family of Mrs. W. J. Breed.

We are making efforts here to have a law passed by our Legislature to prevent the use of alum in baking powders. Respectfully yours, etc.,

E. S. WAYNE, Ph.D., M.D.

In this connection I would state that the Board of Health of Brooklyn, after carefully investigating the adulteration

of baking powders with alum, at a meeting of the Board, March 25, 1879, the following was adopted:

Whereas, Careful analytical examination of the various baking powders sold in this city proves several of them to contain alum; and,

Whereas, The introduction of alum into the human system in this form is injurious to health; therefore,

Resolved, That the Legislature be requested to pass an act prohibiting and punishing the use of alum in baking powders, or in any other mixture intended to be used in the manufacture of bread or other article for human food.

By order of the Board,

H. A. LAFETRA, *Secretary.*

ALBUMINURIA WITH HEALTHY KIDNEYS.

DR. EDLEFSEN, in the *Mittheilungen f. d. Verein Schlesw.-Holsteinsch. Aerzte,* 2, 1879, draws attention to the occasional occurrence of albuminuria where the kidneys undoubtedly are healthy and remain so, as is shown by the fact that the albuminuria is only transient, and always occurs after violent muscular exercise, as, for instance, rapid walking, while the urine is free from albumen after rest. It occurs chiefly in anæmic individuals, and usually disappears after a prolonged use of iron. Bearing in mind that in the cases reported by Leube muscular activity likewise caused, or at least favored, the separation of albumen from the blood, and its secretion by the kidneys, the author next asks what effect muscular exercise has on the general circulation, and particularly on the blood pressure in the renal vessels. If the increased flow of blood outward, caused by muscular activity, diminishes the supply to the internal organs, and consequently the pressure in the arteries of the renal glomeruli, then the more prolonged the muscular exercise, the more marked will be whatever change in the secretion of urine a diminished pressure in those vessels may produce; and in people who, in consequence of anæmia or any other cause, suffer from a relative insufficiency of the heart's action, the proportion of the blood supply to the kidneys during muscular activity will be still less favorable. Now, Runeberg found that, contrary to general opinions hitherto held, in the filtering of albuminous solutions through animal membranes, albumen will pass through faster the less the pressure in the filter, or the greater the resistance around it, and this fact may account for those in any other way apparently unexplainable cases. The author closes with the remark that, as life insurance companies at the present time, with propriety, inquire whether the urine is free from albumen or sugar, the examining physicians should, where albumen is found, always try to discover whether it might not be in consequence of prolonged muscular exercise.—*Medical and Surgical Journal.*

PROGRESS OF INDUSTRIAL CHEMISTRY.

BRIEF REVIEW OF THE MOST IMPORTANT CHANGES IN THE INDUSTRIAL APPLICATIONS OF CHEMISTRY WITHIN THE LAST FEW YEARS.

By J. W. MALLET.

Niter.—An important addition to our knowledge on the much examined but still obscure subject of nitrification has been lately made by the researches of Mueller, Schloesing, Muentz, Warington, and Storer, in reference to the influence of a living ferment in producing this peculiar form of chemical change in nitrogenous material. At the same time new observations have been made upon the range of temperature within which nitrification takes place, upon the unfavorable influence of light, the importance of the presence of calcium carbonate or equivalent basic material, the circumstances under which a nitrite rather than a nitrate is produced, etc. These observations and experimental results will undoubtedly admit of useful application whenever the manufacture of nitre in the so-called "beds" has to be resorted to. Some of the facts recorded by Warington in regard to the influence of light and temperature are quite in accord with results afforded on a larger scale by niter beds established in the Southern States during the war of 1861–5. The manufacture of ordinary niter from Chilian sodium nitrate, begun in Germany on a larger scale for the supply of Russia during the Crimean war, has become an established industry, the saline residue from the vinasse of beet-root molasses distillation and the potassium chloride from Stassfurt carnallite being in turn used as the source of the potassium—the latter much the more extensively at present. To no small extent has this process permitted the purchase of Indian saltpeter to be dispensed with, practically removing it from the market of several countries of Continental Europe.

Potassium chloride.—One of the greatest of new industries based upon saline products is certainly the manufacture of this compound from the great beds of carnallite, crude sylvine, etc., overlying the rock salt of Stassfurt and Kalucz. The process is essentially the simple repetition of crystallizations from solutions in which the double salts of magnesium and potassium are broken up, but in its details the method involves nice attention to the varying character of the crude saline mass treated, to the strength of the solutions at various stages, to the temperature, and to the time permitted for crystallization. In the regulation of temperature, steam-jackets to the pans and coils of steam pipe furnish most useful aid, as they do in a host of other processes of modern chemical industry. Much judgment has, moreover, been shown in determining the extent to which the process of purification may be profitably carried in order to supply the different demands of chemical manufactures and of agriculture.

Potassium sulphate.—This salt is also obtained from the material furnished by the Stassfurt beds, and on a much larger scale than was practicable before the discovery of these valuable sources of supply. It has been made by the action of sulphuric acid upon potassium chloride, in decomposing pans like those used in the first stage of the Leblanc soda process, and turning out a product analogous to the common "salt cake," but to a much larger extent from the potassium chloride of carnallite by double decomposition with the magnesium sulphate occurring in adjoining beds as kieserite, or obtainable by the action of water on kainite from other portions of the same remarkable series of deposits.

It is not yet twenty years since these potassium salts were

but sparingly obtained by-products from the evaporation of sea water and from kelp working. They now represent of themselves important branches of industry, and have served to give new vigor to all manufactures in which potassium compounds are necessary materials.

Potassium chlorate.—The production of this salt has acquired increased importance from its fast extending use in calico-printing and in medicine, as well as in pyrotechny and the making of some kinds of friction matches. The only method of producing it, by passing chlorine into a hot solution of potassium carbonate, has become quite obsolete, and is replaced by the action of chlorine upon milk of lime, followed by conversion of calcium chlorate into the corresponding potassium salt by means of potassium chloride, thus avoiding the measurable waste of potassium by conversion of a dearer into a cheaper salt (carbonate into chloride) to the extent of five-sixths of the whole amount used, and rendering the process at the same time more manageable and well defined. It is interesting to observe that the use of lime was first suggested (though not quite in the way afterwards proposed by Liebig and now practiced) by Graham, whose name we are much more accustomed to associate with researches in pure chemistry and physics than with technical improvements, though for the importance of the latter he seems ever to have had a lively interest.

Potassium py o-chromate.—For this there is also increased demand owing to extended and varied use, chiefly in dyeing (logwood black, etc.), preparation of pigments for painters' use, and for pigment printing (with chrome yellow, vert Guignet, etc.), and as an oxidizing agent. New sources of supply of the raw material for its manufacture, chromite, have been opened in various directions, including localities in Australia, New Zealand, and New Caledonia. The chromite of western North Carolina does not seem to be yet worked, probably on account of defective means of transportation. The use of lime in the roasting of the ore, replacing in part, or sometimes altogether, the potash or more expensive niter formerly used, proves of much advantage by preventing clear fusion of the alkaline material with subsidence of the heavy ore out of reach of atmospheric oxygen, while potassium sulphate affords the means of converting the calcium salt formed into one of potassium.

Potassium ferro-cyanide.—The history of this salt for some years past has been one of declining production for more direct use in developing dyers' and calico-printers' colors, Prussian blue having been in large measure replaced by the coal tar colors and ultramarine; but, on the other hand, of increased consumption in making potassium cyanide for the purposes of the electro-plater and photographer. The immense loss of nitrogen, at least 75 per cent. of that contained in the animal matter used, and no small loss of potash, leave this manufacture in the list of those still much needing improvement. The main conditions for diminishing the waste of organic nitrogen being a high temperature, thorough exclusion of air, and rapid heating up of the solid materials introduced into the furnace, it would seem worth trial whether advantage would not be derived from a mechanically regulated feed of very small charges rapidly following each other into a Siemens furnace with non-oxidizing atmosphere. This salt has within the last few years come to be a minor secondary product of the Leblanc process as applied to making potassium carbonate from the Stassfurt chloride. Recent patents for the manufacture of potassium ferro-cyanide from ammonium sulpho-cyanate of gas liquor, or from the same salt made by the action of ammonia on carbon disulphide, seem to be still in the experimental stage only.

Potassium cyanide.—The manufacture of this now largely used compound presents nothing specially new since Liebig's process—fusion together of potassium ferro-cyanide and carbonate—came generally into use, save that soda is often added, and so a mixed product of potassium and sodium cyanides is obtained. No economically successful process has yet been secured for making this or other cyanides from atmospheric nitrogen, in spite of the numerous attempts at producing such a result.

Sodium chloride.—With ever increasing consumption of common salt, there is little to note of recent important changes in the methods of preparation. In working the mother liquors from the Mediterranean "salt gardens," the necessity for great storage reservoirs, in which to keep the concentrated solution over until it may be crystallized in the cold weather of winter, has been obviated by the introduction of machinery for the production of artificial cold; but the importance of working these mother liquors has been much diminished by the products having now to compete with those from the Stassfurt deposits.

Sodium nitrate.—The very simple process of solution and crystallization by which the "Chili saltpeter" of South America is obtained admits of little modification, but the work has been systematized of late years and carried out with improved mechanical arrangements and upon a much larger scale than formerly, so as to provide for the greatly increased demand both for agricultural and manufacturing use.

Sodium sulphate.—The most interesting advance in the manufacture of this salt, so largely in demand for modern glass making, has been its production by the mutual decomposition of common salt and kieserite (magnesium sulphate), both furnished by the Stassfurt beds. The purity of the product gives it the advantage over ordinary "salt cake" for the glass manufacture.

Borax.—Greatly increased production is chiefly to be noted, due mainly to the discovery of new sources of supply of the raw material, partly the boro-natro-calcite ("tiza") of South America and the boracite of Stassfurt, but much more the great quantities of crude borax and "tiza" of California and Nevada. Extending use of the salt is observable chiefly in the manufacture of pottery and in connection with washing, but this is hardly in proportion to the greatly increased abundance and lower price.

Ammonium chloride.—The recent proposal of Gerlach deserves attention—namely, to work the "ammonia soda" process with a view to producing sal-ammoniac, making the sodium carbonate obtained a secondary product only, and not regenerating the ammonia. To this end gas liquor, or that from bone black works, is to be simply distilled—best in some such apparatus as that of Solvay—the concentrated distillate mixed with brine and treated with carbon dioxide gas under pressure, the precipitated acid carbonate of sodium removed, some ammonium carbonate recovered from the residual solution by distillation, and ammonium chloride then crystallized out, separating it from remaining common salt. A similar process is proposed for making ammonium sulphate and nitrate. It may be worth noticing in relation to these salts, for which the demand is constantly increas-

ing, that enormous waste of ammoniacal liquor prevails, or did but a few years back, at most of the gas works of the United States.

Calcium chloride.—Attention has already been drawn to the great importance of devising a workable process for economically making from this salt and from the corresponding magnesium compound hydrochloric acid or free chlorine. To the list of many other industries which tend to turn out more and more calcium chloride as a waste product may be added salt boiling in certain regions, as in Ohio, where the mother liquors are loaded with it.

Magnesium sulphate.—The various methods formerly depended upon for the production of this salt have lost interest in view of the immense masses of kieserite found in the upper portion of the Stassfurt beds, this requiring only somewhat prolonged treatment with water to get it into solution, and subsequent crystallization. Although used for a larger number of purposes than heretofore, including sundry applications requiring a cheap soluble sulphate, the price is very low and the available supply in excess of the demand.

THE GREAT CHEMICAL PRODUCTS AT THE UNIVERSAL EXHIBITION OF 1879.

THE SODA INDUSTRY.

SINCE the time of the Vienna Exhibition the methods of manufacturing soda have increased in number, and the old process only holds its own through the numerous improvements that have been introduced in it. Such improvements are in great part due to the labors of Pelouze and Scheurer Kestner, Goss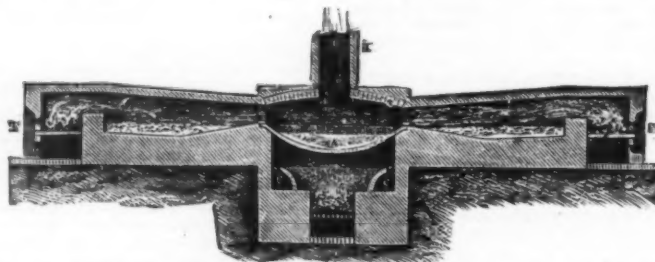age and Dubrunfaut, and the consequent abandonment of the old theories based on the Leblanc process. To replace the latter, a method was needed that should be at once economical and hygienic, and which should yield the soda directly from sea salt through double decomposition. Advantage has been taken of the reaction of bicarbonate of ammonia on sea salt, and which is expressed by the following equation:

$$NaCl + (NH_4)HCO_3 = CO_3NaH + NH_4Cl.$$

By this process the use of leaden chambers has been, in part, done away with; sulphate and soda furnaces have been completely abolished; as well as all the apparatus for condensing the acid vapors; and, finally, the mass of refuse which poisoned the surroundings of the factory has been got rid of, and a saving of about half in fuel has been obtained. As a by-product there remains nothing but calcium chloride, derived from the regeneration of ammonia. Unfortunately the ammonia process furnishes no hydrochloric acid. Now, the latter product is, at the present day, indispensable for the fabrication of bleaching chlorides. It is true that M. Solvay has recently tried the substitution of magnesia for lime; and afterward, by decomposing the chloride of magnesium by steam, he obtains hydrochloric acid, which condenses, and magnesia, which re-enters indefinitely into the manufacture. Should this reaction be realized in an industrial way, such a method would be perfect, from a theoretical as well as from a practical point of view. The first attempts to apply the process to ammonia date back as far as forty years ago. Numerous patents for the application of this process have been taken out since 1838; and one of the principal of these is that of MM. G. Dear and E. Hemming, in common with MM. Grey and Harrison. In France, the first industrial attempt was made by MM. Th. Schloesing and E. Rolland, who, in 1855, improved the process, both as regards its industrial and its scientific aspect. Unfortunately this experiment became involved with a question of legislation and impost.

A perfect industrial application of the method was effected in 1867 by M. Solvay at his Couillet factory (near Charleroi). His production in 1872 amounted to 4,000 tons. Since that time other factories have been started in England, France, and Hungary. The production at the time of the Universal Exhibition of 1878 was, in M. Solvay's four establishments, 7,500,000 kilogrammes of soda salt per annum. The production at Dombasle-Varangéville has arisen in a year to 20,000,000 kilogrammes. In England (at Norwich and Sandbach) it has been 13,000,000 kilogrammes. Finally, several modifications have been introduced into the Solvay process, and other soda works have sprung up. Among others, we will cite the Boulvard process, which is used in the Griffon works at Sorgues (Vaucluse), where the production in 1877 was 3,600,000 kilogrammes. But before going into details, we will rapidly pass in review the most interesting improvements that have been made in the Leblanc process, dwelling particularly on the regeneration of sulphur and manganese.

SULPHATE OF SODA.

The transformation of sea salt into sulphate of soda is still usually effected by sulphuric acid. The majority of manufacturers have substituted muffle furnaces for the old style of furnaces known as "Bastringues." In the former, the reaction takes place in close muffles, heated to a temperature of about 650°. This arrangement allows of coal being substituted for coke. The hydrochloric acid being purer is more easily absorbed, and the yield is better. (See Fig. 1.) Notwithstanding these modifications the labor connected with these sulphate furnaces is very fatiguing to the workmen, and it is becoming more and more desirable, not only as a question of health but also as one of economy, to substitute machinery for manual labor. With this idea in view, we call particular attention to Messrs. Jones & Walsh's furnace, which is in use at Middleborough. This furnace is circular, and the stirring of the mass is effected by scrapers which revolve about a central shaft moved by machinery.

The products of combustion from a coke fire place traverse the furnace and maintain the temperature at 420°. The saving in sulphuric acid is, at least, 5 per cent, and the condensation of the gases, in spite of the mixture of the products of combustion with the hydrochloric acid, is effected very well by means of the condensing carboys and towers that are usually employed. (See Fig. 2.)

The Hargreaves and Robinson Process.—This process permits of the sulphate of soda being obtained directly by means of the sea salt without the use of sulphuric acid, and consists in causing sulphurous acid and steam to act simultaneously upon the salt, and give rise to sulphuric acid, which produces sulphate of soda just in measure as it is formed:

$$2NaCl + SO_2 + O + H_2O = SO_4Na_2 + 2HCl$$
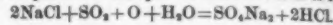
The bruised rock salt is agglomerated into small particles, either by means of damp refined salt or by steam coming from the escapements of the factory engine. These agglomerations are then dried in stoves heated by steam; and afterward distributed through the cylinders by means of endless chains provided with buckets. All this work is done mechanically. The cylinders are of cast iron lined with brick; they communicate with each other, and may be heated by separate fires. They are divided into two parts by a grate forming a false bottom, and which supports the saline agglomerations. The sulphurous acid derived from the direct combustion of pyrites penetrates and mingles with the moist air under the grate. The flow of gas through the cylinders is effected by aspirators; and it is methodical, that is to say, the richer the gas is in sulphurous acid the more transformed salt it meets with, and *vice versa*; or, in other words, the gaseous current pursues a course opposite to that

FIG. 1.—REVERBERATORY SODA FURNACE, WITH ITS ROASTERS.

A, Retort heated by the fireplace, b. F F, Fireplaces for heating the two roasters. H, Flue for leading the gas to the condensing tower.

of the salt. The condensation of the hydrochloric acid is performed in carboys and towers, as in the Leblanc process, but the large quantity of foreign gases renders it a little more difficult. At first, but four cylinders were employed; but the number has been increased to eight, and even to sixteen, thus materially augmenting the expense of fitting up the works. The reaction in this process, although theoretically very simple, is practically a very slow one, and it is very difficult to have a regular temperature. The cylinders are heated

FIG. 2.—CONDENSING TOWER.

A, reservoir of water. C, escape flue for uncondensed gases. D, gas chamber in which the liquid hydrochloric acid is collected. T T, condensing carboys.

only to about 400°, the heat produced by the chemical reaction being more than sufficient to bring about a decomposition of the salt. However, despite the cost of apparatus, the inventors think they will produce the sulphate at a cost of 3 francs 80 (about 75 cents), while even with the latest improvements introduced into the old process the cost is reckoned at 6·10 francs to 6·20 francs ($1.20 to $1.40) per 100 kilogrammes (220 pounds). The production of sulphate of soda by the Hargreaves process amounted in 1877 to about 30,000 tons. Since that period some other factories have adopted the same process.

FERMENTATION IN ITS HOUSEHOLD RELATIONS.

THIS is a subject, said Prof. Brewer at the recent Connecticut Agricultural Convention, that has not been as yet exhausted by scientific men. Much is known of fermentation, but much also remains to be found out. Chemical substances may be divided into two classes, organic and inorganic.

Inorganic substances may be composed and decomposed artificially, but organic compounds can, as a rule, only be decomposed by artificial means. Organic compounds contain carbon, hydrogen, oxygen, and nitrogen. Nitrogenous compounds are known by their offensive smell when decaying. Organic substances are subject to change or destruction through the agency of ferments. Ferments act in solutions, but not in dry matter. Dry sugar will keep perfectly for years, but sugar in water, or in cider, or grape juice, soon changes to other substances. This change is produced through the influence of ferments or yeast. Yeast is composed of living cells which grow and multiply often with great rapidity. These cells are microscopic, and are like little sacks filled with fluid in which granules are floating. These granules are supposed to be the nuclei of new cells. Boiling yeast kills it. The yeast cells of cider have a character of their own, and their work causes the development of acetic acid. The yeast plant or ferment peculiar to milk changes the sugar of milk into lactic acid. Fermentation tears down, never builds up. The yeast particles are very small, but the spores or seeds of these cells are a great deal smaller and may float in the atmosphere, which explains how fermentation may be started in exposed substances without special aid from man. He then described the several processes of bread making, as by alcoholic fermentation, by yeast, baking powders, and by aeration, each of which may be commendable in practice according to the skill used in its manufacture. Potatoes added to the sponge or dough make the fermentation go on more evenly. The function of yeast is to change starch into sugar and that into alcohol. The alcohol thrown off in the process of baking may be collected, but such collection has not as yet been made at a profit. The quality of the bread is injured. Digestion is a fermentative process. The saliva is a ferment, so is the pepsin or gastric juice of the stomach. It is believed that all starchy foods are more easily digested after fermentation. Cabbage is much more digestible in the form of sourkrout than unprepared, because it has passed through a process of fermentation. Yet nobody likes sour bread, nor is it wholesome. He showed samples of vinegar, one made in a clean cask, the other with a small piece of rotten wood thrown in. The first was clear, sharp, and good, while the other was worthless. Some other ferment had gone in with that rotten wood, which had destroyed the vinegar, showing how a poor article may sometimes be accounted for when old musty barrels are used for keeping the cider. He spoke decidedly against the use of vinegar made from sulphuric acid. It should always be avoided if one would retain good health. All fermented drinks contain some alcohol. Small beer may not contain a great quantity, but it contains some, and it is the result of this same ferment. It had been claimed that fermented swill was better for hogs than unfermented, and he was inclined to favor the belief from a few experiments made by himself, though probably the fermentation must not be carried very far.

The preservation of fruit and vegetables by the canning and sealing process is only a method of killing ferment spores, and excluding others that might get in. Boiling salt water is better than fresh in which to immerse the cans, because the salt water will take a higher degree of heat, and thus destroy the ferment germs more thoroughly. Salicylic acid has been recommended for preserving milk from fermentation or souring, and it may prove harmless, but possibly some people may sacrifice health or life in proving the contrary. It sometimes takes a long while to test such substances, because of their slow action. Milk rooms that are not kept clean are sources of constant ferment growth.

Another class of ferments are called septic ferments, or those which accompany putrefaction of animal matter. It is still a question whether all putrefaction is the work of ferments, but it is certain that much of it is, but their precise nature is but poorly understood on account of the extreme minuteness of the particles. It seems a fact that putrefactive germs exist everywhere in the air at ordinary temperatures. Tyndall has made experiments by which he was enabled to obtain perfectly pure air, and in which fresh meats would keep for an indefinite period, but the slightest contact with air having putrefactive germs floating in it set the work of decay in operation. The action of all antiseptics is that of killing germs or ferments. Arsenic preserves the skin of stuffed birds by killing the ferments. Heat is also a great preventive of septic fermentation. Many if not all epidemic diseases are the work of putrefactive ferments. Ordinary poisons kill a man by stopping the life functions, but zymotic ferments kill by propagation and increase of themselves at the expense of the live material upon which they work. To prevent the spread of zymotic diseases, as small pox, diphtheria, typhoid and other fevers, it becomes necessary to destroy all putrescent material. Sink drains, cesspools, neglected privies, and decaying bodies, are all dangerous to health. Copperas water used freely on decaying filth is an excellent disinfectant, and its use should be more common.

THE SUPPOSED ARTIFICIAL PRODUCTION OF THE DIAMOND.

To the Editor of the London Times:

SIR: I should be obliged if you would accord me space in the *Times* in order that I may answer a great number of letters and applications which have pursued me during the past few days on a subject of some little public interest, that subject being the asserted formation of diamonds by a gentleman at Glasgow.

Some ten days ago I had heard nothing whatever of the claim of Mr. MacTear, of the St. Rollox Works, Glasgow, to the artificial production of the diamond. My name, however, was already in several newspapers as that of a person in whose hands the asserted diamonds had been placed for a decision as to their true nature. Ultimately a small watch glass, with a few microscopic crystalline particles, came into my hands for this purpose from Mr. Warington Smyth, and subsequently a supply came to me direct from Mr. MacTear. I shall proceed to state the results I have obtained from the examination of these.

Out of the first supply I selected by far the largest particle, one about ⅟₆th of an inch in length, and it may be that I wasted some time in experimenting on this particle, as it might not have been an authentic example of the "manufactured diamond" since it differed in some respects from the specimens I have since received direct from Mr. Mac-Tear.

Now, firstly, the diamond excels all substances in hardness. Secondly, its crystals belong to the cubic system, and should not, therefore, present the property of doubly refracting light. Frequently, however, from the influence of strain within the crystal, caused by inclosed gas-bubbles or other causes, diamonds are not entirely without action on a ray of polarized light sent through them. Finally, the diamond is pure carbon, and, as such, burns entirely away when heated to a sufficiently high temperature in the air, and more vividly so burns or glows away when heated in oxygen gas.

The specimens I had to experiment upon were too light to possess appreciable weight, too small even to see unless by very good eyesight or with a lens, yet were, nevertheless, sufficiently large to answer the three questions suggested by the above properties.

A few grains of the dust, for such the substance must be termed, was placed between a plate of topaz—a cleavage face, with its fine natural polish—and a polished surface of sapphire, and the two surfaces were carefully "worked" over each other, with a view to the production of lines of abrasion from the particles between them. There was no abrasion. Ultimately the particles became bruised into a powder, but without scratching even the topaz. They are not diamond.

Secondly, some particles, more crystalline in appearance than the rest, were mounted on a glass microscope slide, and examined in the microscope with polarized light. They acted each and all powerfully in the manner of a birefringent crystal. It seemed even in one or two of them that when they lay on their broadest surface (it can scarcely be called a "crystal face"), a principal section of the crystal was just slightly inclined to a flattish side of it in a manner that suggested its not being a crystal of either of the orthosymmetrical systems. Be that as it may, it is not a diamond.

Finally, I took two of these microscopic particles and exposed them to the intense heat of a table blowpipe on a bit of platinum foil. They resisted this attempt to burn them. Then, for comparison, they were placed in contact with two little particles of diamond dust exceeding them in size, and the experiment was repeated. The result was that the diamond particles glowed and disappeared, while the little particles from Glasgow were as obstinate and unacted off as before. I had previously treated the specimen I have alluded to as the first on which I experimented by making a similar attempt in a hard glass tube in a stream of oxygen, and the result was the same. Hence I conclude that the substance supposed to be artificially formed diamond is not diamond and is not carbon; and I feel as confident in the results thus obtained from a few infinitesimal particles that can hardly be measured, and could only be weighed by an assay balance of the most refined delicacy, as if the experiments had been performed on crystals of appreciable size.

Not content with merely proving what these crystalline particles are not, I made an experiment to determine something about what they are.

Heated on platinum foil several times with ammonium fluoride they became visibly more minute, and a slight reddish white incrustation was seen on the foil. At the suggestion of Dr. Flight, assistant in this department, a master in the craft of the chemical analyst, these little particles were left for the night in hydrofluoric acid in a platinum capsule. This morning they have disappeared, having become dissolved in the acid, and on evaporation there is seen a slight white incrustation, on the capsule, of the residuary fluoride. I have, therefore, no hesitation in declaring Mr. MacTear's "diamonds," not only not to be diamonds at all, but to consist of some crystallized silicate, possibly one resembling an augite, though it would be very rash to assert anything beyond the fact that they consist of a compound of silica, possibly of more than one such compound.

The problem of the permutation of carbon, from its ordinary opaque black condition into that in which it occurs in nature as the limpid crystal of diamond, is still unsolved. That it will be solved no scientific mind can doubt, though the conditions necessary may prove to be very difficult to fulfill. It is possible that carbon, like metallic arsenic, passes directly into the condition of vapor from that of a solid, and that the condition for its sublimation in the form of crystals, or its cooling into crystal diamond from the liquid state, is one involving a combination of high temperature and high pressure present in the depths of the earth's crust, but very difficult to establish in a laboratory experiment.

I am, Sir, your obedient servant,

NEVIL STORY MASKELYNE.

Mineral Department, British Museum, Dec. 30.

WORKING THE SPECULUM FOR THE THIRTY-SEVEN-INCH REFLECTOR.*

THE speculum recently finished for the 37-in. reflector gave me an opportunity of coping with the difficulties to be encountered in making a large speculum of this kind. I have found some of these difficulties to be less than I anticipated; and I believe considerably larger instruments might be undertaken with a reasonable prospect of success. There never was any doubt whether large glass specula could be as easily mounted as metal specula, for they have the great advantage of being lighter; but the chief question was whether they could be annealed so as to stand the usual treatment in working. If they would stand this, there is no fear of their durability when mounted. To decide whether the speculum should be metal or glass was, to a certain extent, to venture a risky experiment. For my own part, I had reason to believe that large disks could be obtained properly annealed. Two firms had guaranteed me disks of three or four feet, and since that time they have offered to undertake a 5-ft. disk. One obstinate fact, and one I could get no solution of, was the failure of the 4-ft. French speculum. But, in spite of this failure, a 37-in. glass disk was decided on, to be of about 4½ in. thickness.

There was the question, too, of silvering. Mirrors are usually silvered by being suspended face downward; but I was quite sure that it could be done face upward and without even taking it from its cell, and that I could thus avoid a great difficulty with respect to silvering. I had long been in the habit of silvering mirrors face upward when testing them during the figuring, as it saved both time and trouble, and I consequently devised a plan for silvering the large mirror, which unfortunately, was the cause of the blowing up of the first disk. The cell was perforated as if for a Gregorian. I was going to cut a 2½-in. hole through the glass, and make a gutta percha plug to fit. This hole was to serve to let off the water and solutions when silvering. A band of stiff paper, coated with something not acted on by the chemicals, was to form a band to hold the solutions. I had tried many things; beeswax is good when pure, but is mostly solid adulterated with oil and rosin; solid paraffin was found the best of all. I made a strong machine suited to my method of working. I had flatted the back of the disk and got the concave ready for fining, and was, at the same time, edging it and cutting the hole through the center; but when less then ⅜ in. in depth had been cut, it burst into hundreds of fragments. The breaking, doubtless, was owing to the disturbance of internal tension. I was not sure whether removing the skin from the edge or disturbing the center was the cause of the blowing up, and I therefore tested the question on an 18½-in. disk, which, on attempting to cut a hole through it, blew up also. I have since had a large disk cast with a center in it, and it answers perfectly; and had the 37-in. been cast in the same manner, it would have answered the proposed plan for silvering. The 37-in. was ground and polished with a tool 36 in. in diameter, and weighing nearly 3 cwt., and the focus—17 ft. 7½ in.—within an inch or two of what was intended.

Before polishing was commenced, a portion of the workshop was covered with calico to exclude dust, room only being left for working. A wooden tunnel, commencing at the door and extending 40 ft. on the ground level, was covered with sailcloth to keep out light and air-currents. The machine was made so that its revolving table could be turned from the horizontal to the vertical position; the speculum was worked in its cell; the whole weighing nearly 11 cwt. When testing commenced I sat in this dark tunnel to test at the center of curvature, and the speculum was focused by an assistant with screws, so that I received in my eye the image of a very minute pinhole in a lamp screen close by my head. During the polishing and figuring I carefully studied the behavior of the disk—for flexure—for distortion of figure by contraction and expansion during changes of temperature—and I found the disk as perfect as a 6-in. one, and without a single infirmity. I soon found it would admit of a perfect and permanent figure, and it gave in the early stage a perfect and symmetrical image of an artificial star, which was magnified many hundred times by an eyepiece. These results were gratifying and satisfactory; they at once removed all doubts of ultimate success.

The work of correcting was tedious and trying, especially in the latter stages, when for every few minutes' polishing the whole preparations for testing had to be repeated, and the settling of the mass into its normal state had to be patiently waited for, and often days passed before further advance could be made. When the figure had advanced as far as necessary for testing at the center of curvature, the wooden structure was thrown open at the outer end for the purpose of testing on a distant terrestrial object. The test objects for daylight purposes consisted of a hole punched through a sheet of metal, with a reflector so placed at the back as to reflect the light of the clouds through, so that it appeared as a bright white spot. A spot of whiting and printed paper was used for definition. When the sun shone I obtained its image by means of a prism with one of its surfaces polished to a small spherical curve. This was made use of because I could not use a bulb, having to look in the direction of the sun, and not with the sun behind me. For testing at night a lamp covered with a metal screen with a hole through it was used. For every advance toward the correct figure it was first tested at the center of curvature, and then on the distant object. In the latter case it was, of course, used as a telescope proper, by placing a plane at its working focus for parallel rays. The plan of figuring was that of local figuring and correcting. The polish and surface was obtained with the large and heavy polisher, and corrected with a number of polishers of various sizes and forms to suit every stage of the progress and the temperature of the air, etc. If an error of irregularity of figure set in, it was polished out with the large polisher.

The machine I employed was on a principle which was a modification of that used by the Earl of Rosse. I have made five different machines, one of which was on the principle used by Mr. Lassell—an excellent principle. But I have long since come to the conclusion that no machine can do the final work like the trained hand, and I was gratified to learn, when in conversation with Professor Draper, that his experience agreed precisely with my own on this point. The fact that, when polishing specula, we use a tool that is somewhat elastic (for there is nothing equal to pitch as a material for the polisher) shows that is the form of the polisher we want to aim at, and the curve of the glass will follow. Therefore, the machine, its rates of revolutions and strokes, the size and weight of the polishers, their consistency (which depends on the temperature in which they are worked, and the friction) should all be arranged so as to give to the polisher the figure or curve we desire to give the glass surface. I may state that the 37-in. disk, as an experimental one, answered all expectations, and fulfilled all necessary conditions; and I may also state, as the result of my experience, that I see no obstacle to the construction of glass mirrors of very large sizes.

The 37-in. was silvered by Martin's process, and in this way: The surface was washed with some of the potash solution, it was then rinsed and sponged with a handful of pure cotton wool, and finally rinsed. A band of stout brown paper was ready; that part of it to go next the edge for about two turns was painted with hot melted paraffin. This paper band was wound round, and tightly bound round with strong cord, leaving a rim standing up about two inches above the surface to hold the solutions. Water was then poured on to cover and keep the surface till the bath was ready. The solutions were filtered through cotton wool. I make the solutions very strong; that is, I dissolve the chemicals in one-quarter or one-eighth the quantity of water usually given. Thus the filtering is quickly done and the bulk is small. The quantity of water required to make the bath of sufficient depth can be easily added. The bath was about one inch deep at the edge. When all was ready, the speculum was tilted and the water shot off; quickly brought to the level again, and the solutions poured on. The sinking particles are thus subtracted by filtration, and the light particles will float harmlessly, and nearly all the silver goes to the glass surface. It was well silvered in twenty minutes, then well washed and sponged over with a handful of cotton wool, finally rinsed, left to dry and polish. It was conveniently silvered with the help of one assistant.

CELESTIAL OBJECTS VIEWED WITH THE NAKED EYE.*

THERE are many persons possessing a love for scientific subjects who relinquish all idea of ever doing any useful work because they have not the means to procure expensive

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* By G. Calver, in the *Monthly Notices* of the Royal Astronomical Society.

* By W. F. Denning, F.R.A.S., in *Science for All.*

and elaborate instruments. They imagine that in order to cope with their contemporaries it is necessary to be similarly provided with the best appliances for observation and experiment, and a well-stocked library of new and standard works for reference. This is particularly the case in astronomy. A man thinks, when his attention is first attracted to the subject, that the colossal telescopes existing in observatories in various parts of the world have already made all the possible discoveries, and that a puny, indifferent instrument such as his own is incapable of displaying anything new or revealing much of what is previously known. He begins to despair, and when he has seen a few of the chief objects dimly portrayed in his small glass becomes dissatisfied, and ultimately it is laid aside as his thoughts enter a new groove, for he takes up a different hobby holding out a better chance of success and pleasure. This has been the case many times, and will be so again; but there is no need to say that a person may accomplish very useful work by the proper application of the means at his disposal, and that even without any instruments at all there is a large amount of valuable astronomical data to be collected and many sights to be seen that shall fill the spectator with genuine interest. In fact, there are many celestial appearances which are only observable with the naked eye, for they will not admit of examination in the contracted field of a telescope. The chief thing necessary to success is a great love for the subject, for this is required to sustain the enthusiast through his nightly vigils, and will lead him to devise the means by which his ends may be best attained, and will point him to the particular department in which he is likely to achieve something useful. Unremunerative labor tires most men, but the true student of science will make any sacrifice of time and labor in the development of his observations or theories. Not that a man need necessarily let his scientific work interfere materially with his business avocations. In the evening, after the day's toil, rest may be found in the contemplation of celestial objects, and when the subject is pursued in moderation it tranquillizes and elevates the mind, and affords a welcome relaxation and change. And such observations, while thus proving beneficial, may become of real value to science if the observer records a careful and accurate account of what is displayed before him. Eclipses of the sun and moon, apparitions of the aurora borealis and zodiacal light, variable stars, shooting stars, comets, and many other objects and phenomena come within range of the unassisted vision, and require much further watching and description. To such observations as these, extending over a long period of years, Professor Heis almost entirely owed his great reputation as an astronomer. Telescopes he might have used, and of the first excellence, had he wished, but he preferred to rely upon his eye alone; and his extensive publications, embracing star atlases and catalogues, the paths of many thousands of shooting stars, observations of the zodiacal light, etc., are a sufficient testimony of his unwearying energy, and an incentive to less ardent students in the same field. It is true that instruments are so much improved, and have multiplied to such an extent during the last few years, that an astronomer without a telescope would be deemed quite a curiosity at the present day; but it is certain that, however much optical aid is necessary for some kinds of observation, it is possible to discern many natural beauties with nature's own unaided vision, and thus to gather many new facts from the rich stores of space. . . . One of the most remarkable of naked eye observations was that by Moestlin (Preceptor of Kepler) of eleven, and perhaps fourteen, stars in the Pleiades, commonly called "the seven stars." To ordinary vision six stars only are visible, but the telescope has displayed a large number of fainter ones scattered amongst the group. On December 24, 1579 (nearly three years before the invention of the telescope), Moestlin determined the positions of eleven of them, and they are found to coincide with the points now more exactly ascertained. The writer has frequently looked at this well-known asterism, and finds thirteen stars usually visible to the naked eye. On the night of December 6, 1876, when the atmosphere was exceptionally transparent, and the stars shining with remarkable brightness, fourteen were detected.

This group forms a specially interesting object, and is favorably visible throughout the autumn and winter months. The smaller stars are very faint, and the observer should remember that a minute object is often glimpsed by averted vision when steady, direct gazing fails. Any one who cannot succeed in seeing these smaller stars of the Pleiades will have no chance in observing the satellites of Jupiter with the naked eye, for though they have been undoubtedly detected without telescopes, yet they are very faint, and being immersed in the planet's rays, are almost wholly overpowered, except at the time of greatest elongation, when two of them (the third and fourth) being occasionally in conjunction, afford a capital opportunity for testing the vision. These little moons are generally in a line with each other, though not invariably all visible, for they suffer numerous eclipses and allied phenomena. As to Jupiter himself, he is often perceptible in daylight. Bond has often seen him with the naked eye in high and clear sunshine, and the sunrise. Venus is always a very conspicuous object in the daytime, when her position is sufficiently distant from the sun. The writer has frequently seen this planet at noon, shining very strongly, and she has been similarly noticed by many people. In fact, there is no difficulty whatever in seeing this beautiful planet in the daytime, if the position is pretty well known, and care is taken to make the observation from a place where the sun's direct rays are intercepted and cannot dazzle the eye.

Another object of some interest to naked eye observers is the middle star (Zeta) in the tail of Ursa Major, which has a small companion, named Alcor, close to it. It was called "Saidak" by the Arabs, signifying "the Tester," for it was customary amongst them to test a man's power of sight by it. Humboldt, in his "Kosmos," says that he has seen the smaller star with great distinctness every evening on the rainless coast of Cumana, but has recognized it only rarely and uncertainly in Europe. Observers may, however, find no difficulty in seeing the star, for it is a remarkably easy object, and, at the present time, certainly no test of vision. It may possibly have become brighter than it formerly was, for it is now extremely plain, even in unfavorable conditions of the atmosphere. There is a third and fainter star near it which really forms a very difficult object to reach with the naked eye.

There are some stars, affording good examples of proximity (though differing little in magnitude), which require care in separating them without resorting to instrumental aid. The chief star (Alpha) in Capricornus consists of two stars of the third and fourth magnitudes, about six and a quarter minutes of arc asunder (corresponding to one-fifth of the moon's diameter), and readily distinguished as a double star. Epsilon Lyræ presents another (though more difficult) instance of like nature.
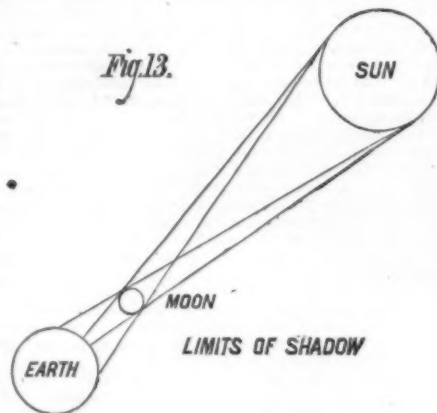
THE SUN.

By S. P. LANGLEY, Allegheny Observatory, Pa.[*]

A "TOTAL" ECLIPSE.

EVERY one has seen an eclipse of the sun of some sort, but a "partial" eclipse as seen through a piece of smoked glass, though no doubt a curious and interesting, can hardly be called an imposing phenomenon. From some such experience, perhaps, many form an idea of what a "total" eclipse may be like, but in reality there is hardly any resemblance. Not only is a solar total eclipse, by general agreement, the grandest and most imposing spectacle nature offers, but it is to most the rarest of all; the chances being against any average human life's bringing the opportunity to see one from any given place on the earth's surface.

Besides this it is a most important opportunity for seeing certain things about the sun which are never visible even to the most powerful telescope at any other time. We say "about," and not "on," advisedly, for the things in question belong to a region extending out from the sun into space, where every feature is usually obliterated by the greater brilliancy of sunlight. It is only when this is withdrawn, and we are in the shadow of the moon, that the "corona" appears, though it is always existing there; as the stars are by day in the heavens unseen till the shadow of the earth makes night. When such an event as a total eclipse occurs, observers therefore travel if necessary across the globe to see it, though the spectacle lasts usually less than five minutes; and one such is to appear in the Territories of the United States on the 29th day of the present month (July, 1878).

It will be seen from the annexed figure (Fig. 13) that when

Fig. 13.

SUN

MOON

EARTH   LIMITS OF SHADOW

the moon comes between the sun and earth, two shadow cones are formed; one (the larger) within which the observer will have his view of part of the sun cut off by the intervening body (and see a "partial" eclipse), the other cone marking the limits within which the whole sun is rendered invisible, and the eclipse is total.

The first cone grows larger and larger as we go away from the moon in the direction opposite the sun, the second smaller and smaller. If the moon were a little further off than it is, the apex of this second cone might be reached without its touching the earth at all, and as her distance is variable this in fact sometimes happens. The moon is always so far away (and so small compared with the sun) that the section of the inner cone where it strikes the earth is at all times small, or, in other words, the part of the earth whence a total eclipse can be seen is never more than a very small portion of the whole. The section of the inner cone where it strikes the earth is (where the sun is vertical), generally speaking, a circle of less than 200 miles in diameter, and as this section is carried along by the moon's motion and the earth's together, it sweeps over the surface of our globe in such a narrow belt as is shown in Fig 14, which is taken from the *American Nautical Almanac*, with a very

to a new sense of the reality (if I may so speak) of the heavenly bodies, for the moon, which we have been accustomed to see as a disk of distant light on the far background of the starry skies, takes on the appearance of the enormous solid sphere which it is, and a faint glow within its circumference (due, perhaps, to reflection from the corona) makes its rotundity so perceptible that we feel, perhaps for the first time, the perpetual miracle which holds this great cannon-ball-like thing from falling. But almost at the same moment we become aware that its immense shadow is rushing toward us, blotting out the landscape, and advancing like a material darkness with an effect actually terrifying.

Lest I seem to exaggerate, let me quote the words of another, a trustworthy and careful witness. Principal Forbes, watching the eclipse of July, 1842, in Europe, says of this: "I perceived in the southwest a black shadow like that of a storm about to break, which obscured the Alps; it was the lunar shadow coming toward us. Those who have seen a locomotive approach at the rate of 40 miles an hour can judge of the stupefaction caused by the approach of this black column with all but lightning speed. I confess it was the most terrifying sight I ever saw. As always happens in the case of sudden, silent, unexpected movements, the spectator confounds real and relative motions—I felt almost giddy for a moment, as though the massive building under me bowed on the side of the coming eclipse."

Another witness, Captain Biddulph, says: "The light cloud I saw distinctly put out like a candle. The rapidity of the motion of the shadow, and its intenseness, produced a feeling that something material was sweeping over the

Fig. 14.

60

40

20
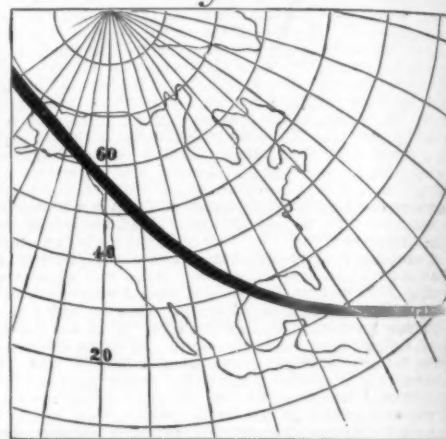
REGION OF TOTAL ECLIPSE JULY 29. 1878.

earth at a speed perfectly frightful. I involuntarily listened for the rushing noise of a mighty wind."

The shadow having involved us, we look up to the place the sun occupied a moment ago, and find in its stead a black circle, around the edge of which are irregular flames, or what seem like flames, chiefly of a rose red, rising in fantastic shapes to heights which in some cases have exceeded 80,000 miles (Fig. 15). These are not always present in equal quantity. In the eclipse of this month they will probably be few, but they are always a beautiful spectacle. The illustration annexed (Fig. 15) is taken in part from a paper in the notices of the Royal Astronomical Society, describing the English observations of an eclipse in India, and gives a fair idea of the sizes of these "flames" compared with that of the sun. The variety and in some cases beauty of the "flames" themselves, when studied separately by the spectroscope, are very great, and even as small as the scale of the drawing is, they exhibit great diversity of outline. None are here seen entirely detached from the sun, and floating cloudlike above its surface, but such are sometimes visible. At the time of the eclipse at which this

"ECLIPSE ENCAMPMENT."

slight modification that the heavy black line across the continent marks both the track along which totality lies in the width of the very narrow region through which alone it is visible.

When from an elevated station we watch the progress of a total eclipse, the sun's disk is seen to be slowly invaded by the advancing moon, and as the solar brightness is gradually reduced to a thin crescent, daylight fades with increasing rapidity, and a quite peculiar and unnatural light, hard to describe but which no one forgets who has once seen it, spreads over the landscape. Then, and suddenly, we come

drawing was taken, the "flames" were the objects of principal curiosity, and it was even uncertain till then whether they were attached to the sun or moon. But the dark body of the moon was distinctly seen to advance over them, and their fluctuating character was exhibited by drawings taken a short distance of time apart. Thus the great prominence at A is shown on an enlarged scale at A 1 with its curious twisted structure as it appeared to the English observers at Guntoor, while at B is another enlarged view of the same prominence as it appeared at Mantawalock which the eclipse reached later. It is very plain that its form has altered in the interval. The curious spiral, striated structure of A has also been observed by Professor Abbe of the Uni-

ted States Signal Service, in portions of the corona itself, or in what appears to be such. The whole structure of these red "flames" allies them with the delicate cloud forms described here as seen in spots, and it will likewise be noticed that they are shown on the figure as not being seen about the solar poles, a region from which the spots are also about. Beyond them, stretching out into space for distances sometimes equal to the sun's entire diameter, are brushes of pale light, whose extremities some describe as perceptibly curved and scintillating, or at least fluctuating. (These were to me the most striking thing in the eclipse of 1869.) It is not entirely certain how far these brushes are a real

Fig. 15.
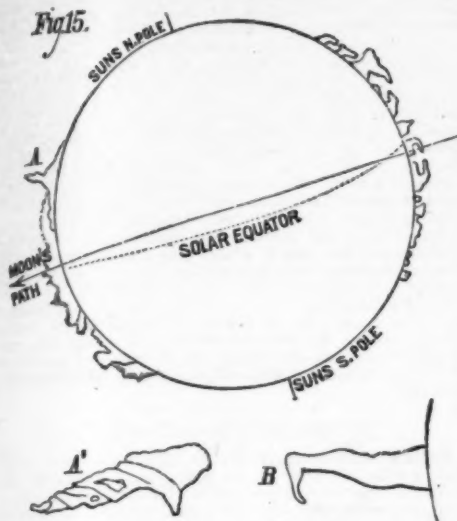
solar appendage, for something like them can undoubtedly be produced by the rays of the sun broken by the ragged mountainous edge of the moon, and seen reflected from the distant parts of our own atmosphere, in such a way that by an effect of perspective they seem to be entirely without it (Fig. 16). Nearer to the body of the invisible sun the light grows brighter and more continuous, till close to the black moon it becomes much brighter than full moonlight would be, and gives so much light, that in the complete absence of the sun only the brighter stars are visible. The darkness is then by no means absolute, and it is further lessened by light reflected from regions in the extreme horizon, which are without the limits of totality.

The red flames are a part of what is called the chromosphere of the sun. The rest of the appearances described belong to the corona, the crown or glory about the eclipsed orb, as they seem, looking, in fact, much like the aureole represented by painters about the heads of saints. Fig. 16 represents the inner corona and red flames as drawn by Professor J. H. Eastman, U. S. N. Fig. 17 is from a sketch by Tacchini, and shows the more extended corona rays as seen at the eclipse of December, 1870. The total phase lasts at the longest six or seven minutes, but rarely as much as that. In the case of the eclipse of 1870, observed at the station of Xeres de la Frontera, by the U. S. Coast Survey eclipse expedition, the whole duration was two minutes and ten seconds, and for the opportunity afforded by this brief interval the ocean had been crossed by a whole body of observers. Two principal parties were dispatched for the purpose by our Government, and the operations of that at Xeres, under the direction of Professor Winlock, may be taken as an example of the care and preparation used on such an occasion.

The party in this case consisted of fourteen, eleven of whom were from this country, and the station (in a vineyard near the town of Xeres), presented from the number of the tents, the appearance of a military encampment (p. 3412). Every variety of instrument that science uses at such a time was in requisition : huge telescopes, solidly mounted and driven by clockwork, carried photographic apparatus ; others spectroscopes ; close by was a heliostat and horizontal telescope 40 feet in length, also for photography. Other

Fig. 16.

INNER CORONA (EASTMAN)

telescopes were directed so as to form cameras, for sketching the corona ; still others bore polariscopic apparatus for determining the character of its light. Elaborate provision for measuring its brightness was made, and in charge of a little division of the party in a neighboring orange grove, while a coast survey transit station had been improvised, with mounted transit, chronograph, chronometers for determining the time, and telegraphic connections established for the purpose with the Spanish Observatory of San Fernando, near the city of Cadiz. Each of the observers had drilled himself for weeks beforehand in every part of every observation to be made by him, and there was such subdivision of labor

that each had one thing only to do. As the critical moment approaches, lamps are lighted. Clouds are sweeping over the sky, and it has been raining a few minutes before, but now a break in the clouds appears about the sun, showing the light dwindled to that of the thinnest crescent moon. A moment more and darkness seems to come suddenly down ; the groups of spectators around the confines, kept back by sentinels, grow indistinct ; an utter silence settles with the falling night, and for two minutes is unbroken ; and while this silence lasts it may be safe to say that every observer has every faculty of mind so busy that will serve his work bent on it with such a tension as one must have experienced to understand. Even while it seems to each as if he had only begun to do what he wishes to accomplish, something seems to whirr past overhead, and turning the eyes we see the retreating shadow flying over a distant chain of mountains, which bounds the eastern horizon, while the sun's rays come out as suddenly and as dazzlingly as those of an electric lamp. It is over, and what those brief minutes have yielded is being written down by each one, while every impression is fresh in mind, before he leaves his post. This done, voices break out again ; there are comparisons, congratulations, and regrets ; but nothing can be done now but to leave the work as it stands, with the feeling, we may hope each has, that he at any rate did his best, whether that turned out much or little.

What knowledge have such labors brought us ? Not nearly all we could wish, it must be confessed, for the corona is still in great part a mystery. The spectroscope shows that its light consists largely of a line in the green part of the spectrum, very nearly coinciding with an iron line ("1474, Kirchhoff.") It was for a time inferred that it might be largely composed of iron vapor, but this supposition, never a very probable one, has been disproved by Professor Young, who with one of the recent very powerful "grating" spectroscopes has shown that the coronal line is not identical with the iron one, as supposed, and that the collocation probably means nothing. It is very difficult to see how the corona can be vapor of any sort, for the reason, among others, that comets have been known to pass through it without any visible effect on these excessively light bodies.

The most recent investigations seem to point to the conclusion that the corona is composed of an infinitude of minute discrete particles, somewhat like a dust cloud, but intensely hot from the neighborhood of the sun. This leaves us, however with the difficulty of determining why these particles do not fall upon the sun's body. Now we must either suppose them to be moving about it, like other bodies obeying the law of gravitation (and there are several difficulties in the way of this), or suppose them to be kept away from

Fig. 17.

OUTER CORONA (TACCHINI)

the sun by a repulsive force. Fanciful as this last supposition may seem to be at first sight, it is not to be dismissed as impossible in the present state of our knowledge, which is leading us to look on the existence of such forces co-existently with gravitation (such as we familiarly know it) as even probable. It is to be hoped that our coming eclipse may do much toward settling such questions.

The "red flames" which have been spoken of are an object of far minor importance at an eclipse than formerly, because, according to the well known discovery of Messrs. Janssen and Lockyer, they can be made now visible at any time without an eclipse by the spectroscope. They constitute, however, a beautiful addition to the eclipse phenomena, and one point at least may be noted during the totality which the spectroscope alone could not tell us of—that they are not all of the rose red mentioned, but that occasionally some of a more nearly orange hue have been seen, thus marking the presence at great heights above the sun of other substances than hydrogen, to which the principal color is due.

It may perhaps be of interest, in view of the immediate approach of the eclipse, to give the approximate times of its occurrence in some of the principal cities where this will be read. By help of the computations of our Nautical Almanac Office we find :

Place.	Local time of beginning.	Of ending.
New York	4h. 47m.	6h. 33m.
Philadelphia	4h. 43m.	6h. 31m.
Baltimore	4h. 38m.	6h. 26m.
Cincinnati	3h. 58m.	6h. 02m.
Chicago	3h. 42m.	5h. 41m.
Pittsburg	4h. 20m.	6h. 12m.
New Orleans	3h. 51m.	5h. 52m.
San Francisco	0h. 52m.	3h. 17m.

It is possible that this article may fall into the hands of some one residing where the eclipse is total, and who may wish to be able to make some observation of service. None is likely to be so useful as a drawing made on the spot—not "corrected " or "improved" by subsequent retouching—and accompanied by any remarks as to the features of the corona seen by the naked eye or with the telescope. These may be sent to the Naval Observatory, Washington. A large number of parties will visit the eclipse track, and many of them have started with the beginning of the month to be early in the field and to make every preparation on the ground. Among these are several European astronomers of high re-

putation, and it seems at present as if no previous eclipse is likely to have received the attention to be bestowed on this. We must not forget how completely all are at the mercy of the weather at such a time, and how large the risk is which all take that their labor and time will be rendered valueless, by an hour of cloudy sky ; and remembering this, we will wish for all clear weather and full success.

(To be continued.)

THE SUN.

In the current number of the *Princeton Review* Prof. Charles A. Young sums up the results of recent investigations and studies of the sun. Within the past few years substantial progress has been made in the problem of the solar parallax. Until about twenty years ago the accepted value of this element was about 8·58", corresponding to a distance a little greater than 95,000,000 miles between the earth and the sun. Various researches made between 1850 and 1860 showed that a considerable correction was necessary, and different estimates were made bringing the parallax for the most part between 8·80" and 8·90", corresponding to distances of 92,9 0,000 and 91,800,000 miles. The value 8·85" was deduced by Prof. Newcomb in 1867, and this, or 8·86", has been used by the Germans, Americans, and French in their Ephemerides, while the value 8·95" has been employed by the *British Nautical Almanac*.

During the past five or six years these figures have been believed to be too large, and, while the observations of the transit of Venus have as yet given no decisive results, there have been made within the last two years two determinations of the solar parallax, which are thought to be the most trustworthy yet obtained. One of these values, deduced by Mr. Gill from his observation of the opposition of Mars at Ascension Island in 1877 is 8·78", corresponding to a distance of 93,100,000 miles between the earth and sun. Within the past year, Mr. Albert Michaelson, of the United States Navy, by what appears to be an extremely accurate measurement, has determined the velocity of light to be 186,370 miles, with a probable error of less than five miles. From this velocity and Struve's constant of aberration may be deduced for the parallax the value 8·81". Prof. Young considers it "quite safe to conclude from the present state of the data that 8·80" is nearer the true value of the sun's parallax than 8·85", and most probably the final result will turn out even a little below 8·80"." Hence the distance of the sun may be put at about 93,000,000 miles.

Investigations made within a few years show that the amount of heat emitted by the sun and the temperature of the sun are much greater than they were formerly supposed to be. While Secchi and Ericsson have deduced a solar temperature of several million degrees, French astronomers have contended for a temperature of only about 2,000° centigrade. Both of these estimates are now regarded as being far from true. In this country Professor Langley has lately proved that the sun's temperature is much higher than that of the fiercest artificial heat, while Rosetti, of Padua, has computed the solar temperature to be 10,000° centigrade, or 18,000° Fahrenheit, "a result," says Prof. Young, "which would seem to be much more worthy of confidence than any previously given."

An important addition to our knowledge of the chemical constitution of the sun has been made within the past two years. Prior to 1877 the solar atmosphere was not known to contain any but metallic elements, hydrogen being classed among the metals. In 1877 Dr. Henry Draper, of New York, discovered oxygen in the sun's atmosphere, and was thus the first to demonstrate the presence of a non-metallic element. The recent investigations of Mr. Lockyer and the discussion of the theory advanced by him mark an epoch in solar physics. His researches tend to show that "the non-metallic bodies, and, indeed, all our so-called elements, are not really elementary, but compound, and that many of them cannot exist in the sun, because the solar temperature is so high that they would be split up into their constituents." This conclusion has been the subject of earnest discussion and dispute among physicists and chemists, but it has been received with considerable favor, and the evidence in support of it seems to be accumulating.

The progress recently made in the application of photography to solar observation has become very marked. All the delicate details of the photospheric structure are shown in photographs of the sun having a diameter of more than eighteen inches. By these pictures, some remarkable facts relating to the surface of the sun have been brought to light by M. Janssen, of the observatory at Meudon, near Paris. While this observer and Dr. Draper have accomplished much by their photographic studies of the sun's surface and spectrum, Capt. Abney, of England, has carried the photography of rays at the lower end of the spectrum far beyond anything before attained. If his methods shall prove practically successful, it is expected that the lower invisible part of the spectrum may be studied with the same ease as the ultra violet.

Prof. Young considers the signs of future progress in solar physics very encouraging. The Meudon observatory is doing much photographic work which promises fruitful results. An "astro-physical" observatory, to be devoted chiefly to the study of the sun, has been established at Potsdam by the German Government. Some of the most distinguished specialists of Germany are connected with it. The University observatory at Oxford, England, is also largely employed in solar study.—*N. Y. Sun.*

PROF. COLLIER ON SUGAR MAKING.

At the recent Agricultural Convention at Willimantic, Conn., Prof. Peter Collier, of the Department of Agriculture, Washington, made an address, which is reported as follows in the *New England Farmer :* Prof. Collier, in introducing his subject, said that few can realize the magnitude of the sugar interest in America. According to statistics, the United States is importing, annually, sugar to the value of $109,000,000, and since the year 1848, when gold was discovered in California, it would take all the gold and silver from all the mines in the country to pay for the one item of imported sugar. Commissioner LeDuc had been called crazy by some on the subject of sugar making, but he was himself willing to be considered even more so, for he felt sure from the results of experiments he had been making during the past year, that five years hence the United States would produce her own sugar. We have the material already, and we have only to use it. Progress in the arts and sciences may often be marked by the utilization of waste products. There is corn enough grown in one-eleventh of the State of Illinois to produce all the sugar now imported into the country, and of the whole crop of the United States it would require but 1½ per cent. to supply the sugar purchased. Most of his experiments had been made with sorghum cane, a sugar plant sufficiently hardy for culture

throughout a large portion of the country. There are some fifteen to twenty varieties of sorghum, four of which he had planted during the past season on the department grounds. Frequent analyses had been made during the summer of the juice of the stalks, and he had found as high as 17 per cent. of crystallizable sugar in the best specimens, while of the uncrystallizable product there was less than 1 per cent. in the best specimens. Sugar cane received from New Orleans showed but 12 per cent. of sugar. These experiments were not made on a small scale, for over 250 separate determinations had been worked out during the summer.

One difficulty experienced by the sugar makers of Louisiana is in the large waste of sugar in the process of manufacture. It is estimated that only about one-half the sugar contained in the cane is utilized, while by his experiments he had been able to secure full 92 per cent. of the sugar from the sorghum cane in his nine best determinations, and over 90 per cent. in the nine poorest.

Referring to Prof. Goessmann's experiments, he said he thought that he must have followed an imperfect method by which a portion of the crystallizable sugar became inverted. The apparatus for making sugar from corn and sorghum is quite inexpensive, as a mill and fixtures costing $150 will do for several farms; and any person of ordinary intelligence can learn to manufacture the sugar. The Company at Crystal Lake, Michigan, had sold the past autumn 42,000 pounds of raw sorghum sugar at ten cents per pound, and the Company will continue the business next year. In his own experiments he had made an average of over one ton per acre, while in Louisiana 1,350 pounds of sugar per acre is considered a fair average product.

The Professor was quite certain that two tons of sugar is not more than may be depended on per acre, from the best varieties of sorghum cane grown in the latitude of Washington, and that there will be about two months in which to do the work of harvesting and manufacturing. At the present time we are producing but thirteen per cent. of all the sugar we consume, leaving eighty-seven per cent. to be imported from other countries, chiefly from Cuba. This is entirely too great a draught upon our present resources. Speaking of sugar from cornstalks, the Professor said that if we can learn to save the sugar now wasted in our cornstalks, it would be like eating our cake and having it too, for the stalks contain a large amount of good sugar after the corn is sufficiently matured for grain. The speaker next gave statistics showing that the yield per acre of the corn and wheat crops of most of the older Western States is gradually growing less, and that unless some change is made in our system of culture the country will soon be growing perceptibly poorer. Here, at the East, our wheat crop is better by the acre now than at the West, simply because we fertilize our land, yet we do not begin to manure our land as does England. That little island, only a trifle larger than our State of New York, imports annually twenty million dollars worth of fertilizers in addition to the large sums paid for feeding stuffs, such as cotton seed and linseed meal, imported largely on account of their manurial value. North Carolina, in order to grow her present crops, is compelled to buy more than two million dollars worth of fertilizers, and all the other states that are exporting agricultural productions to England or elsewhere, are gradually depleting their soils of fertility.

From analyses of twenty-eight samples of corn it is found that this grain contains 1·63 per cent. of mineral matter, largely phosphoric acid and potash. This seems like a small amount, and yet for the whole yield of the country it will require ninety-nine million dollars to supply the corn crop a single year with these minerals at present market prices. Now, as the value of the entire crop when grown is about four hundred and eighty millions of dollars it will be seen that about twenty per cent. of its value is required annually for restoring its ash ingredients. Is it to be wondered at that thinking men are a little anxious as to the future of our agriculture as related to our national wealth and independence? Now, sugar does not draw upon the land, but is, to a great extent, a product of the atmosphere, and if we should export a thousand million dollars worth in a year, as he believed we might do, it would not impoverish our soil to any appreciable degree. Sweet corn may be grown for the grain to be canned at the factories, and then the stalks may be made to yield a crop of sugar of equal or even greater value, making a double crop, the stalks freed from their juice still being of value as food material for cattle. Maine, last year, put up corn to the value of one and one-half million dollars, and might have saved an equal value from her corn stalks had she been so disposed.

The feeding value of corn does not lie wholly in its sugar content, although that is of value for food. The pressed stalks have been found by analysis to contain a larger percentage of nutriment than the unpressed, pound for pound; or in other words, 100 pounds of stalks weighed after the juice has been mostly pressed out contains more starch, and on the whole is of greater feeding value than 100 pounds of stalks with the water and sugar all in. He had little doubt that the new method of preserving green fodder in pits or silos would work much to our advantage in this business of sugar making from corn stalks. Of the exact relative value of sweet corn compared with other varieties for sugar making he was not prepared to state definitely. He had, the past summer, raised of Dent corn a crop equal to sixty-nine bushels per acre, and from the stalks made sugar at the rate of 1,000 pounds per acre.

SUGAR FROM CORN AND SORGHUM.

The speaker gave some account of the processes adopted for making sugar from sorghum and corn stalks. A two horse mill will press out seventy-five gallons of juice per hour. The juice is collected in barrels or tanks, from which it is taken as wanted to the defecating pans for purification. When heated to a temperature of 82° C., cream of lime is added in quantity just sufficient to render the juice, which is at first a little acid, slightly alkaline, the test being made by litmus paper. The impurities are coagulated and rise to the surface in a scum or settle at the bottom, the liquor in the meantime having been brought to the boiling point. After this it is cooled to 84° C., when nineteen-twentieths can be drawn off as a clear liquor. This is then reduced by boiling to a heavy puffy state, when it is ready to draw into pans for crystallization. In an old second-hand pan he had been able the past summer to evaporate 175 gallons of the juice per hour. Alluding to the relative value of the beet for sugar making, he said it must be remembered that the manufacture of sugar from beets had not been carried to a very high degree of perfection in the old country. Beet culture and sugar making had been encouraged by government subsidies, without which the business would have been less profitable. It is an accepted fact that, unless everything works well in the manufacture of sugar from beets, the business is a losing one,

while cane will pay for working, even where, as in Louisiana, but half the juice is expressed. Comparing cane and beet culture in this country, he felt very confident that the cane both from sorghum and corn would come out ahead. Our country is very much better adapted to the growth of corn than of roots of any kind. Roots require a moist atmosphere, while corn will only succeed in a hot dry climate. Europe is adapted by her climate to root culture, while the United States is the home of the corn plants. Not two per cent. of the farmers in this country are at present sufficiently educated in root culture to grow roots successfully, while in England ninety-eight per cent. grow them as a staple crop. And the reverse is about equally true of the corn crop, as applied to the two countries, for everybody can grow corn here and almost nobody there. He gave it as his opinion that cultivators inexperienced in root culture would make it cost five times as much to grow an acre of beets as an acre of good corn. In growing beets for sugar making it is not desirable to obtain the largest crops possible, for such roots are less rich in sugar. Twelve to fifteen tons per acre give the best quality of beets, while sorghum will yield more tons per acre and afford a juice richer in sugar than ordinary sugar beets.

No lecture during the week created more interest among the listeners than this by Prof. Collier, but it remains to be seen how many of his predictions will prove true. There seems little doubt, however, that the next five years will bring a revolution in the sugar industry of the country.

JAMES R. NAPIER, F.R.S.

We announce with no small regret that Mr. J. R. Napier died at his residence in Glasgow, on Saturday morning, in the 59th year of his age. The eldest son of the late Mr. Robert Napier, of Shandon, the famous shipbuilder, Mr. Napier was long associated with his father—first as an assistant and afterward as a partner. Leaving the firm of Robert Napier and Son, he subsequently embarked in shipbuilding on his own account at Govan; and it was while thus engaged that, in order, if possible, to settle the continued disputes between employers and workmen, he gave practical shape to a scheme for personally interesting his *employés* in the business. Although this experiment was not crowned with success, it was not the fault of Mr. Napier, and the fact of his attempting it was but the outcome of that untiring thirst for improvement which gave motive and shape to everything he undertook. Mr. Napier was a member of the Association of Shipbuilders, and was largely instrumental in promoting the union between that body and the local engineers' society, and his efforts led to the establishment of the Institution of Engineers and Shipbuilders in Scotland, of which he occupied the office of president, and to whose transactions he was for long a prolific and valued contributor. He was also for many years associated with the late Professor Macquorne Rankine in the production of a variety of useful inventions in marine and general engineering. The Philosophical Society of Glasgow had no more active or useful member than James R. Napier, and he was recently the chief means of instituting a new department of that society known as the Physical Section. He was indeed all along one of the leading spirits of the Philosophical Society, and a great and successful advocate of the division of that body into sections, as the best means of conducting its business. After he had been for some time in business as a shipbuilder along with Mr. Thomas Hoey, Mr. Napier again became connected with the firm of Robert Napier and Sons, and this connection brought him into contact with all that was best and most advanced in the arts of shipbuilding and marine engineering, of the practical as well as the scientific aspects of each of which he had the most accurate personal knowledge and experience. But Mr. Napier cared less than most men for the emoluments of his business; his whole thought and the continual bent of his disposition was in the direction of invention and improvement, and he desired these for their own sakes much more than for that of any pecuniary advantage he might be likely to derive from them.

We have already alluded to his connection with the Institution of Engineers in Scotland, and it is but simple justice to say that that body was greatly indebted to him for the position it occupied and the advantages it was the means of conferring upon the Scotch engineers, situated as they were at so great a distance from the headquarters of other and greater societies of a similar character. Mr. Napier was for many years a member of the Institution of Mechanical Engineers, and when that institution visited Glasgow, not on the last occasion merely, but a good many years ago, it was mainly through his exertions that the meeting was made a success. He was president of the Scottish Institution of Engineers from 1863 to 1865, and it was while he occupied that post that, in April, 1865, a union was consummated, in which he took a leading part, between the Engineers and Scottish Shipbuilders' Association, thus, as we have indicated, forming the Institution of Engineers and Shipbuilders in Scotland, which is now a very useful and flourishing society. Mr. Napier was also for long a member of the Institution of Naval Architects, having been placed on the council of that society when it was instituted in the year 1860. He worked most assiduously with Professor Rankine in the endeavor to develop motive power by means of heated air, and probably nothing he ever tried to accomplish was more thoroughly pursued than this object as long as it appeared to him that there might be the slightest chance of its attainment. He edited, along with Rankine, a large work on "Shipbuilding, Theoretical and Practical," published by Mackenzie, of Glasgow and London, which contains an enormous mass of letterpress in every department of the subjects of shipbuilding and marine engineering, together with many illustrations, which were of great value when they first appeared, and must still be of interest to those concerned.

But, apart from such works as these, undertaken when he was as yet a comparatively young man, Mr. Napier does not appear to have cast his researches in a permanent form. At many of the meetings he attended, and to the proceedings of which he made contributions, his papers and speeches were quite fragmentary. Valuable hints and much light many of them no doubt contained, but if they are ever to be of use to the student of the subjects with which they dealt, they will have to be excavated from the transactions of the different institutions.

It will probably be acknowledged that Mr. Napier was a most useful member of the British Association. He was a regular attender of the association's meetings, having missed only one or two of them, we believe, for the last twenty years. In the public debates in the sections of the association, he gave place, as a rule, to gentlemen who were readier speakers, but he was a devoted committee man. Indeed, from one or other of the committees of investigation of the

British Association his name was seldom missed, and he took an extensive share of the practical work which was necessary to furnish the materials for the reports of the committees. Now that the steel age had been fully entered upon, it may be of interest to record the fact that, as far back as 1847, Mr. Napier designed several steel vessels, stern-wheel steamers of a light build, for the Indian rivers, which were built, if we mistake not, by Messrs. John Elder & Co. A few years ago Mr. Napier invented the Indian pressure log for taking the speed of vessels, an instrument which is manifestly a great improvement upon the old log. Mr. Napier likewise devoted a large share of his attention to the heating and ventilating of buildings on scientific principles. In the autumn of 1876, when the British Association last visited Glasgow, Mr. Napier was intrusted with the organization of an exhibition of mechanical and other novelties in the Kelvingrove Museum, and his services in connection with this exhibition, which was highly successful, were warmly acknowledged at the time. Fully alive to the disadvantages in which inventors are placed by the present patent laws, Mr. Napier worked hard during recent years, along with Sir William Thomson and others, to impress upon the government the necessity of a reform in those laws. With this object they formed the Inventors' Institute, a body which has repeatedly submitted to the Lord Chancellor and other members of the government suggestions on patent law reform. Personally Mr. Napier was a man of plain and unpretending manners and kindly disposition, and was much admired by those with whom he came more immediately into contact, and who were thus in a position to appreciate the value of his genius. In recent years Mr. Napier was in the habit of paying a somewhat extended annual visit to the Mediterranean for the benefit of his health. He leaves a widow and several sons and daughters; two of the former are connected with the firm of Napier, Shanks, & Bell, shipbuilders at Dalmuir.—*The Engineer.*

INTERESTING TO PATENTEES.

Senator Hoar strongly advises patentees not to spend their money in trying to get their patents extended. He says that experience shows that no bill for the extension of any seventeen year patent can pass Congress. The feeling against the extension of patents is very strong. Bills have passed one house or the other, but they are always beaten in the end. He says that if he had a brother who had a patent worth $50,000, he would not advise him to spend $1,000 to get it extended.

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